

OBITUARY NOTICE

ERNEST JONES, M.D., D.Sc., F.R.C.P., 1879-1958

On 11 February 1958 Ernest Jones died in London at the age of 79 years. With his death was severed the last link with that classical period in the development of psychoanalysis when, with the assistance of a small but devoted band of acolytes drawn from Vienna, Zurich, Budapest, Berlin, London and New York, the discoveries of Freud were spread abroad, and psychoanalysis, from being a science with for ten years but one solitary advocate, became a firmly organized movement centred on an International Association. Ernest Jones was the last survivor of this band of conquistadors. In his time he led an ever-multiplying contingent of English-speaking analysts through Canada, New York and London to the four quarters of the British Commonwealth; fought in every battle of offence and defence throughout the pioneering period that lasted almost twenty years (1908 to 1928); and on the death of Freud in 1939 became the acknowledged leader of psychoanalysis throughout the world. During that time he organized and expanded Freud's approaches to the cognate sciences of psychology, sociology, and anthropology, and to the territories of aesthetics, movements which were regarded as unwarranted invasions. And he left behind him a richly documented record of progress as a teacher, research scholar, writer, organizer and finally leader in the field of psychoanalysis.

Born in 1879 in Gowerton, Glamorgan, of Welsh parentage, he proceeded from the village school, where he was recognized as a boy 'of parts', to Swansea Grammar School, thence to University College, Cardiff; and, having later studied at University College Hospital, obtained the degrees of M.B., B.S. at the University of London in 1901. In 1903 he took the senior degree of M.D. (Lond.); in 1904 he became an M.R.C.P. and in 1905 obtained the D.P.H. (Cambridge). With both junior and senior medical degrees he was awarded gold medals. He was elected Fellow of the College in 1942, and in 1954 was awarded an honorary D.Sc. by the University of Wales.

Jones's hospital career had a significant bearing on his ultimate choice of profession. Like many brilliant students who cannot see their way clear to follow the conventional track to Harley Street and the practice of consulting medicine, he lingered in hospital work, taking the usual house appointments in U.C.H. He was also clinical assistant at the Royal Hospital for Sick Children and the Ophthalmic Hospital. More significant were his appointments as clinical assistant at the National Hospital, Queen Square, and as registrar and pathologist at the West End Hospital for Nervous Diseases. These were followed, after a few intermediate hospital posts as assistant physician and physician, by a lectureship in practical neurology at the London School of Clinical Medicine. So that already before he left England for Canada, his interest was fully slanted to neurology, to which speciality he had already contributed some papers (published in *Brain*). In this respect there was a certain parallel between his progress and the early orientations of Freud.

It was, indeed, because of this interest that he went to Munich for post-graduate study in neurology and psychiatry and became acquainted with the writings of Freud. His

absorption in the new science increased when in 1908 he went to Toronto as demonstrator of medicine and psychiatry at the University and psychiatrist to the Toronto General Hospital. Later he was appointed Professor of Psychiatry at Toronto and Director of the Ontario Clinic for Nervous Disorders. By this time he was already employing a psychoanalytic technique in his work and published some of his results in the *Journal of Abnormal Psychology* in 1909.

During his stay in Canada psychoanalysis took up an increasing amount of his attention. With his active support it had already obtained a footing in New York. And when the International Psychoanalytical Association was founded in 1910 he was one of its foundation members. The die was soon to be cast. Encouraged by Freud whom he met during the latter's visit to America in the company of Ferenczi he decided to return to England, and in 1913 did so.

At first a lone wolf, as Freud in the late Victorian nineties had been a lone wolf, Jones was to find support through the accident of international circumstances. The psychological casualties of the First World War had provoked the interest both of psychiatric specialists and of some academic psychologists. The more discriminating amongst these were also stimulated by Freud's theories of the origin of the psychoneuroses. And so after reconnoitring the situation in London for some time Jones found opportunity and support to set up a London Psychoanalytical Society. The future of psychoanalysis in England seemed assured.

But, alas for the prospect, psychoanalysis, being based on the psychology of unconscious conflict, is itself continually prone to fission between its adherents. It soon became apparent to Jones that the members of the newly formed Society were not at one on matters of principle; and on matters of principle Jones was always adamant. So in 1919 he formed the British Psychoanalytical Society which met at first in his consulting room and later in that of Dr Riggall in Wimpole Street. A small group, most of whose members have since died, met together and by a process of co-option and apprenticeship slowly built up the British Branch of the International Psychoanalytical Association, of which latter, incidentally, he was about to become President in 1920, the year in which he founded the *International Journal of Psycho-Analysis*.

Thenceforward the progress of the British Society was increasingly rapid and by 1926 the articles of association of the Institute of Psychoanalysis were signed. The Institute gave formal cover to the British Society, took over the *International Journal* which Jones edited from its inception until 1939, published in conjunction with the Hogarth Press the *International Psycho-Analytical Library*, which under Jones's editorship produced some fifty volumes of mostly standard works on psychoanalysis, undertook under the auspices of its Training Committee (of which Jones was Chairman up to the Second World War) the teaching and training of psychoanalytical students, organized the 'orientation courses' for the medical profession and the general public, and founded the London Clinic of Psychoanalysis of which Ernest Jones was the first Director.

In spite of these many activities, opposition to psychoanalysis in Britain, as elsewhere, remained unabated, was indeed increased owing to the activities of many unqualified persons who called themselves 'psychoanalysts'. Medical uneasiness increased to such a pitch that an investigating Committee was appointed by the British Medical Association. Jones was the sole psychoanalytical representative on a body of about twenty, most of whom were not conspicuous for their support of psychoanalysis. This was Jones's last

gladiatorial display. Not to put too fine a point on it, he fought his critics on the committee, not to mention numerous hostile witnesses, to a standstill; with the result that the B.M.A. Committee refrained from expressing any collective opinion, but laid it down that 'the term "psychoanalysis" can be legitimately applied only to the method evolved by Freud and to the theories derived from the use of this method'. As far as Britain was concerned this was a psychoanalytical charter and definitely marked the close of the pioneering phase.

From this time Jones, who had been President of the International Psychoanalytical Association from 1920 to 1924, played an increasing part in the international policies of psychoanalysis. After the death of Abraham of Berlin he fell heir to the position of leader, was re-elected President of the Association in 1932, a position he held until 1949, when, on retiring, he was elected with acclamation its honorary President. Amongst his many achievements in this field the most important was his conduct of negotiations with the American Psychoanalytical Association which helped materially to maintain the international unity of psychoanalysis.

But these biographical items, impressive as they are, do not convey adequately the status of Ernest Jones as a scientist, as a leader or as a man. As a leader the foundations of his psychoanalytical fame rest firmly on the facts, first, that he defended psychoanalysis during that vulnerable period when it was subject to continuous, violent and often vituperative opposition; second, that through his unremitting efforts he was able to preserve psychoanalysis from internal disruptive influences. Jones had unique qualifications for these tasks. Keen and nimble of wit, he could use his well-stocked mind with devastating effect on those opponents who crossed swords with him. He was a born controversialist with a sharp tongue and a quick eye for the weaknesses of an argument; and he had no mercy on opposition that was based on malice. His counter attacks were not, however, purely polemical in character. He was an accurate, polished and on occasion persuasive exponent of psychoanalysis; having cleared aside the debris of controversy, he could improve the occasion by giving lucid presentations of even the most recondite psychoanalytical theories. The same positive qualities were shown in his leadership. As a Congress Chairman he could with a few trenchant phrases reduce to order the bear-garden confusion of a continental discussion, and at the same time persuade opposing parties to however reluctant compromise.

As a scientist Jones's position was unassailable. Thoroughly versed in psychoanalytic method and at the same time closely conversant with the methodologies of general psychology and sociology as well as those of natural science, immensely industrious, with a quick perception of essentials and a scholar's grasp of their implications, he made an ideal research fellow, and having above all an orderly, even meticulous mind, he was able to dovetail his contributions tidily with the existing structure of both psychoanalytical and psychological theory. Although a great deal of his earlier work was inevitably didactic, he broke new psychoanalytical ground in many directions both theoretical and clinical. The various editions of his *Papers on Psycho-analysis* combined the virtues of a good text-book with the stimulating qualities of a research manual. His work was always accurate and well documented, and although not so profound as that of his master Freud amply compensated for this by its comprehensiveness of range.

But valuable as these contributions were it was in the field of applied psychoanalysis that he gained a reputation amongst the cultivated and discriminating public. Here again, his range of interest was extremely wide and gave him ample opportunity to display his almost obsessive erudition. Always an encyclopaedic reader, with an insatiable curiosity and a quick appreciation of relevant detail, his psychoanalytical contributions to literary and artistic problems and puzzles, to folklore and myth, to various aspects and manifestations of religion, to linguistics and a host of other social, cultural and anthropological subjects were always illuminated by a wealth of apposite instance. Amongst literary critics he will best be remembered by two of his works, his essay on Hamlet and his biography of Freud. He was one of the outstanding exceptions to the general rule that psychoanalysts, although expert to the point of redundancy in building up case-histories, are of their professional nature not very good (that is to say, unbalanced) biographers. Jones's lifelong concern with the unconscious mind did not professionalize or blunt his humanistic perceptions and values. The Freud biography alone, an immense compendium of fact embroidering a sustained characterological and ideological study of one of the world's great men, would be sufficient to secure him a lasting reputation amongst the discerning.

But professional achievement alone, though justifying fame, is not the ultimate criterion of greatness. It is, indeed, a commonplace that truly distinguished men give witness of their distinction in their private as well as in their professional lives. And it can be said without reservation that Ernest Jones was a man of distinction. Although this is a quality that is hard to pin down, one cannot, therefore, refrain from speculating on the personal sources of his professional eminence.

Now the secret of the careers of men lies somewhere between their creative drives and their defences against the more unruly of their passions. Jones was a Welshman born and bred, passionate in temperament, sensitive in feeling and perception, combative as well as impatient in spirit and in his own way endlessly ambitious. Moreover, as a boy he had absorbed the traditions of success that tend to prevail in impoverished and provincial principalities and which have driven the Welsh to frequent eminence in the overlord kingdom of England. Equipped by the accident of birth with good brains he learned at an early age to discipline those more sentimental Celtic accesses that would have impeded his advancement. And it was soon clear that he would achieve success in any career he elected to pursue. All that was lacking was a calling. Neurology indeed tempted him mainly through its demands on his intelligence and on his love of complex problems; but it was not until he fell under the sway of Freud that he developed a consuming passion that could sublimate his more emotional urges.

By the same token psychoanalysis afforded him a disciplined outlet for an unusual degree of filial piety. His scientific allegiances to Freud were immensely reinforced by a personal devotion, which, although it never erred on the side of idolatry, provided him with a private ego-ideal which ultimately developed into an identification. He could not himself have been a Freud, but he could at least conduct, embellish and spread the ideas of his Homeric mentor, and he could at least render tribute to Freud's greatness by every expedient of scientific defence, including the preservation of psychoanalysis from internal erosion and external bowdlerization. It was this urge incidentally that accounted for his

disregard of unpopularity. The well-being of psychoanalysis meant more to him than success in professional diplomacy.

Not only so. It was this devotion to a Freudian ego-ideal that governed his private life and relations. There were not many who were admitted behind the curtain of his professional devotions. Those who were accorded this privilege found him simple, passionate and human, not disdaining the frailties but with few of the foibles of common life. For above all else Ernest Jones was a realist. Indeed, the ultimate secret of his devotion to psychoanalysis was that it enabled him to tie his passion for realism to psychobiological moorings. In doing so he set a standard not only for himself but also for those who follow in his professional footsteps.

EDWARD GLOVER

VISUALLY CONTROLLED LOCOMOTION AND VISUAL ORIENTATION IN ANIMALS*

By JAMES J. GIBSON

Psychology Department, Cornell University

A general theory of locomotor behaviour in relation to physical objects is presented. Since the controlling stimulation for such behaviour is mainly optical, this involves novel assumptions about object perception and about what is called 'visual kinaesthesia'. Evidence for these assumptions is cited. On the basis of this theory it is possible to suppose that animals are visually oriented to the surfaces of their environment, not merely to light as such. In short, it is possible to explain why they seem to have space perception. Implications of this approach for maze-learning are pointed out.

I. INTRODUCTION

The locomotion of animals seems to be controlled by optical as well as by other kinds of stimulation for, in many species, locomotor behaviour ceases when optical stimulation fails, as in total darkness. In bats, to be sure, we now know that locomotion occurs in darkness under the guidance of auditory stimulation, or echolocation (Woodworth & Schlosberg, 1954, 359 f.), but this interesting discovery only serves to emphasize the importance of vision for the majority of animals in the business of getting about the environment, of avoiding obstacles, and of seeking food. Students of animal behaviour have long been concerned with locomotion at the level of reflexes and forced movements, but not at the level of averting collisions or of approaching goals: there is a considerable literature on phototropisms, taxes, and kineses (e.g. Fraenkel & Gunn, 1940), but almost nothing on such acts as the pursuit of prey or the flight from a predator. The reason for this failure is probably connected with the generally accepted assumption that the ability of an animal to respond to *light* and the ability of an animal to respond to *objects* are quite different problems. The first is supposed to be physiological and the second psychological. A tropism implies only a sensory process, but a response to an object at a distance implies a perceptual process and this is much more complex. The physiologist is willing to study the former but he is glad to turn over the latter to the psychologist. The problem of how we respond to objects at a distance has been, in truth, a puzzle for psychologists ever since Bishop Berkeley. Depth or space perception has been taken to be an internal process involving the interpreting or organizing of sensations. Theories of this process have never achieved either simplicity or plausibility and, if it is a process of association, the difficulty arises of why animals do not make more errors than they do in the acquisition of locomotor behaviour. The writer has argued (1950) that the only way out of this theoretical muddle is to discard the original assumption about sensory response to light on the one hand and perceptual response to objects on the other. Perhaps the distinction was based on a mistaken conception of vision in the first place. If the theories of space perception do not provide an adequate explanation for the visual control of locomotion in animals, let us

* The first draft of this paper was written in 1955-6 at Oxford University under a Fulbright award. The author wishes to thank Prof. George Humphrey, members of the Institute of Experimental Psychology, and others at the University for kindnesses beyond the demands of academic courtesy. He also thanks these and many other investigators visited during the year for cordial and stimulating discussion.

forget about the former and pay heed to the latter. Locomotion is a biologically basic function, and if that can be accounted for then the problem of human space perception may appear in a new light. The question, then, is how an animal gets about by vision. How does it react to the solid surfaces of the environment without collision whenever there is enough light to see them by? What indicates to the animal that it is moving or not moving with reference to them? What kind of optical stimulation indicates approach to an object? And how does the animal achieve contact without collision? What governs the aiming and steering of locomotion? (The problem of 'roundabout behaviour' in the presence of a 'barrier', as it appears in the writings of Lewin (1933, 1935) and later Hull (1938, 1952) is a different question. It is as close as psychologists have come, however, to a psychology of locomotion.)

II. POSTULATES FOR A GENERAL THEORY OF THE VISUAL CONTROL OF LOCOMOTION

The starting point for a theory of locomotion in the higher animals would be the fact that they have evolved eyes (Walls, 1942) which can register not merely light but the objects of an illuminated environment. The optical basis for this assertion has been accumulating for years but the conclusion has not generally been drawn despite its having radical implications for a theory of behaviour. The function of a highly developed mosaic of visual receptors is to respond to what might be called the *projective* capacity of light. When light is many-times reflected in all directions from an array of surfaces—when it 'fills' the environment as we say—it has the unique property that reflected rays will converge to any point in the medium. The objective environment is projected to this point. If an eye is placed at this point, it can register a sector of the projection by the familiar process of the formation of an image. The central hypothesis of the theory to be presented is that the *patterns* and the *changes of pattern* of this projection are stimuli for the control of locomotion relative to the objects of the environment. For the sake of explicitness, it might be well to list the assumptions singly. The first six assumptions deal with the projection of light to the position of a motionless animal, that is, with the optic array to a stationary point. The remaining postulates assume an animal in motion and deal with the optic array to a moving point.

(1) *The locomotor environment of animals consists of the surfaces or interfaces between matter in the solid state and matter in a liquid or gaseous state. This generalization holds for aquatic, terrestrial, or aerial environments, and for animals at various phylogenetic levels.*

(2) *Solid surfaces generally reflect rather than transmit light. They also generally prevent rather than permit locomotion. Consequently, the remaining liquid or gaseous regions of the environment constitute a medium both for the transmission of light and for the movements of organisms.*

(3) *If the environment is illuminated there will occur a many-times reflected flux of light in the medium. If the solid surfaces and their parts vary in reflectance a projection of these differential reflectances is obtainable at any point in the medium.* That is, the rays converging to this point will have different intensities (and frequency compositions) in different directions. They constitute what may be termed an optic array.*

* This is a simplified statement, holding for an ideally diffuse illumination. In other conditions of illumination, what we loosely call 'shadows' as well as reflectances must enter into consideration. The extent to which animals can discriminate in their behaviour between shadows and solid surfaces is a complex problem which can here be omitted.

(4) *An eye is a device which registers some portion of the 'pattern' or 'texture' of an optic array to a station point. Conversely, the pattern of an array is a stimulus for an eye.*

This postulate needs explanation. Eyes, to be sure, are of different types in different animals (Walls, 1942). At one extreme, a pair of eyes can include nearly the whole pattern of a 360° optic array, each eye registering an approximate hemisphere with little overlap. This is the case with fish. At the other extreme, a pair of eyes can register overlapping sectors or cones of the frontal optic array, each picking up the same pattern from a slightly different station point. This is the case with primates in whose case the eyes are mobile and co-ordinated, being capable of exploratory (saccadic) fixations and convergences. The total array can then be registered only by rotating the eyes and head. The registration process is successive, not simultaneous, since different angular sectors of the array are picked up at successive moments of time. Nevertheless, by a mechanism as yet not well understood, successive registration seems to be equivalent to simultaneous registration.

The pattern of an optic array was said to be a 'stimulus' for an eye. For animals with fixed eyes on either side of the head this assertion is plausible. Each eye registers its own hemisphere. For animals with mobile eyes at the front of the head, however, the assertion is not so obvious. The total pattern of the spherical array is then, strictly speaking, a *potential* stimulus as distinguished from the *momentarily effective* stimulus which exists for a single eye-posture. The pattern of a single entering sheaf of rays is only a sample of the whole pattern projected to a point in space. But the whole pattern may and will be picked up if the animal looks around. In an even more abstract sense the whole flux of focusable light pervading the medium is a potential stimulus which can be sampled at various station-points in the medium, although it must be explored by locomotor rather than by oculomotor action. It is, of course, unorthodox to assume that the focusable light converging to an eye is something to which the word 'stimulus' should be applied. We have generally believed that only the focused light constituting the retinal image excites receptors. But this physiological conception of the stimulus has been a source of paradox and confusion in psychology. In fact, it does not apply. The image is a stimulus for the retinal mosaic, to be sure, but not for an eye. Focusable light is the stimulus for an eye, which responds first by focusing it. The image is no more than a *response-produced* stimulus. A retinal image is not a thing with definite boundaries in any case. The retina continually moves behind it, with both large and small excursions, so as to bring the fovea to different bits of detail. A continually new sample of the pattern of the world gets projected on the retina. Images are the necessary means by which an animal exploits the capacity of light to project reflecting surfaces to any given point in a transmitting medium. The light converging to such a point is the external, stable, and relatively permanent stimulus for an eye.

(5) *The presence or absence of a solid environmental surface in any part of the optic array is specified by the textured or textureless character, respectively, of that part. In other words, textured light indicates the differentially reflecting structure of a solid surface, which stops locomotion. Untextured or homogeneous light indicates an unobstructed medium of water or air, into which locomotion can proceed indefinitely (Gibson & Dibble, 1952). The presence of a solid object in the environment is specified by a textured cone in the optic array with a*

distinct boundary, and the absence of an object is guaranteed by the absence of such a stimulus (Gibson, 1951).

This assumption, it may be noted, contradicts the classical distinction between sensory and perceptual processes. The present tacit agreement as to where the responsibility of the visual physiologist ends and the responsibility of the visual psychologist begins is thereby destroyed.

(6) *In so far as the physical solids of the environment are rigid and permanent (as contrasted with fluid and movable) the pattern of the optic array at any point in the medium is unique and permanent. That is to say, there is, and will be, one and only one pattern for each station point. It is also assumed that no considerable section of a natural environment is a duplicate of another—that models or copies of envioning spaces are created only by man. An important consequence of this assumption is that the difference between the patterns of the optic arrays at any two station points is a unique difference which depends only on the difference between the points* (Nicod, 1930). We may call it a unique transformation.

(7) *An animal which moves passes through a continuous series of station points. Each eye is therefore presented with a continuous family of transformations, and this family is unique to the particular path of locomotion. With an animal which does not change its position, the eyes are presented with what may be called a continuous non-transformation of the optic array, that is, a static pattern.*

The classical geometry of perspective will describe the static pattern of the optic array, that is, the projection of an environment to a stationary point. The geometry of 'motion perspective' (Gibson, Olum & Rosenblatt, 1955) will describe the 'flow pattern' of the optic array, that is, the projection of an environment to a moving point. The geometry of motion perspective has been exemplified by an analysis of aerial flight with reference to a plane surface. It is interesting to note that the flow pattern is independent of the static pattern that carries it; the same flow pattern may be embodied in any number of static patterns.

(8) *An eye is a device which registers the flow pattern of an optic array as well as the static pattern of an array. Conversely, such a family of continuous transformations is a stimulus for an eye. There are quite specific forms of continuous transformation, and the visual system can probably discriminate among them* (Gibson & Gibson, 1957). *This mode of optical stimulation is an invariable accompaniment of locomotor behaviour and it therefore provides 'feedback' stimulation for the control and guidance of locomotor behaviour. It might be called visual kinaesthesia.* *

The last assumption asserts something like an unrecognized sense modality. Visual kinaesthesia is, of course, supplementary to the recognized mode of proprioceptive kinaesthesia. It differs, however, in several ways. First, it seems to provide information about movements of the animal relative to the environment, not about movements of parts of the body relative to other parts, as the muscle-sense does. Secondly, it seems to provide information about displacements rather than information about accelerations and gravitational forces, as the vestibular sense does. Thirdly, the displacements registered have reference to the stable solid surfaces of the environment; displacements with reference to the medium of air or water, in the case of flying or swimming animals, are given only by proprioceptive kinaesthesia. Kinaesthesia has long been defined as the sense of bodily motion. It was described by Sherrington fifty years ago (1906). It depends on the sensitivity of receptors in the muscles and joints to compression, on the sensitivity of a statocyst to force, and also on the sensitivity of the skin to deformation. Visual kinaesthesia

depends on the sensitivity of a retinal mosaic to an overall change of pattern. To suggest that the eye can serve as a kinaesthetic organ is somewhat upsetting; for one thing it violates our notion of the eye as an exteroceptor in Sherrington's classification of exteroceptors, proprioceptors, and interoceptors. There is some inadequacy, however, in all attempts at anatomical classification of fixed senses. Kinaesthesia, as usually understood, is actually several different forms of sensitivity to several different kinds of motion: of the muscles, the joints, the endolymph, and the skin. Why not admit optical motion? The fact is that animals seem to pick up stimulus information from the sea of energy around them through multiple channels and in various ways. Seemingly redundant information is not neglected; on the contrary, many supplements and concomitants and combinations of energy seem to be received at the same time. And the transformations in the field of view are very useful information.

Consider the case of a fish maintaining its position over the bottom of a flowing brook, or the case of a bird hovering over the same bit of land in a wind. It must move relative to the medium in order to be motionless relative to the solid substratum. For this, obviously, it must rely on vision. It has to maintain an unchanging pattern of the optic array. Transformation of the array is an index of motion relative to the substratum, and a completely reliable one. By visual kinaesthesia the animal can co-ordinate its behaviour to the environment while at the same time by muscular kinaesthesia it can co-ordinate its locomotion to the medium.

Is there evidence to show that the ocular mechanism in animals can actually register the flow pattern of an optic array, as assumed? Animals, including insects, are said to be very sensitive to 'motion', and there is observational evidence from zoology and ethology (Tinbergen, 1951) suggesting that animals make specific responses to specific kinds of moving objects in the environment. If they are sensitive to objective motion in the environment it is likely that they are also sensitive to subjective movement in the environment (Gibson, 1954), but the question has not been specifically studied. There is, of course, plenty of evidence to show that animals have pattern vision in varying degrees, and experiments on form discrimination make a large literature in comparative psychology. Whether they have *change-of-pattern* vision is another question, and this has scarcely been investigated. But it is very likely that they do. The compound eyes of insects, particularly those with a relatively coarse mosaic, may prove to have greater acuity for differences between transformations than they do for differences between forms. There is evidence to prove that human observers are very sensitive to optical motions. They can detect motion parallax in the field of view with very great precision (Graham, 1951), and they can utilize the shadow transformations on a translucent screen for accurate judgments of changing depth (Gibson & Gibson, 1957).

III. FORMULAE FOR A THEORY OF VISUAL CONTROL OF LOCOMOTION

We are now prepared to apply the postulates to the problem of how an animal gets about in a stable environment, approaching goals, avoiding obstacles, and even responding to moving objects such as a prey or a predator. By these assumptions, transformations of the total optic array to a moving point serve for the control of locomotion relative to the total environment. Transformations of a smaller bounded cone of the optic array (a closed contour with internal texture in the visual field of the animal) serve for the control of

ocomotion relative to an object in the environment. A formula will be given for each of the following aspects of locomotor behaviour: beginning forward locomotion, ceasing locomotion, reversing locomotion; steering toward a specific place or object; approaching without collision; avoiding obstacles; pursuit of a moving object; and avoiding a moving object. The formulae will be in verbal rather than mathematical form, but they are capable of the latter kind of statement.

i) *Starting, stopping, and backing-up*

The flow pattern of the optic array during forward displacement vanishes during non-displacement and is reversed during backward displacement (seventh assumption). A human observer who attends to his visual sensations can note a sort of 'expansion' or 'contraction' of the patchwork of colours in his frontal visual field as he moves forward or backward. Actually there is a centrifugal flow of the structure of the optic array from a pole in the direction of displacement, the flow being graded in proportion to the nearness of the corresponding surface (Gibson *et al.* 1955). (This graded centrifugal flow of an optic array from an environment of solids has the interesting property of being capable of continuing indefinitely without the consequence that the pattern as a whole becomes larger. Elements or forms of the pattern are magnified as the corresponding objects get nearer, but new elements emerge to take their place in the array. The structure of such an array consists of forms within forms to an unlimited density. The reason for this fact is that the environment of animals is itself structured at any level of magnitude; it is composed not only of mountains and trees but also of crystals and cells.) There is a corresponding centripetal flow toward a pole in the direction opposite the displacement. These flow patterns depend on the configuration of the reflecting surfaces in the environment but they are independent of any *particular* configuration. To begin locomotion, therefore, is so to contract the muscles as to make the forward optic array flow outward. To stop locomotion is to make the flow cease. To reverse locomotion is to make it flow inward. To speed up locomotion is to make the rate of flow increase and to slow down is to make it decrease. An animal who is behaving in these ways is optically stimulated in the corresponding ways, or, equally, an animal who so acts as to obtain these kinds of optical stimulation is behaving in the corresponding ways.

ii) *Steering and aiming*

The centre of the flow pattern during forward movement of the animal is the direction of movement. More exactly, the part of the structure of the array from which the flow radiates corresponds to that part of the solid environment toward which he is moving. If the direction of his movement changes, the centre of flow shifts across the array, that is, the flow becomes centred on another element of the array corresponding to another part of the solid environment. The animal can thus, as we would say, 'see where he is going'. The act of turning or steering is, therefore, a visual as well as a muscular event. To turn in a certain direction is to shift the centre of flow in that direction relative to the fixed structure of the optic array. The amount of turn is exactly correlated with the angular degree of shift. The behaviour of aiming at a goal object can now be specified (although the properties of a figure in the field of view which arouse this behaviour have not yet been described). To aim locomotion at an object is to keep the centre of flow of the optic array as close as possible to the form which the object projects.

(iii) *Approaching without collision*

Approach to a solid surface is specified by a centrifugal flow of the texture of the optic array. Approach to an object is specified by a magnification of the closed contour in the array corresponding to the edges of the object. A *uniform* rate of approach is accompanied by an *accelerated* rate of magnification. At the theoretical point where the eye touches the object the latter will intercept a visual angle of 180° ; the magnification reaches an explosive rate in the last moments before contact. This accelerated expansion in the field of view specifies imminent collision, and it is unquestionably an effective stimulus for behaviour in animals with well-developed visual systems. In man, it produces eye blinking and aversive movements of the head, even when the stimulus is a harmless magnification of a shadow on a translucent screen. At lesser intensities this 'looming motion', as it might be called, presumably yields lesser degrees of aversion, or a slowing down of approach. The fact is that animals need to make contact without collision with many solid objects of their environment: food objects, sex objects, and the landing surfaces on which insects and birds alight (not to mention helicopter pilots). Locomotor action must be a balance between approach and aversion. The governing stimulation must be a balance between flow and non-flow of the optic array. The formula is as follows: contact without collision is achieved by so moving as to cancel the centrifugal flow of the optic array at the moment when the contour of the object or the texture of the surface reaches that angular magnification at which contact is made.

(iv) *Steering among obstacles*

Fish, birds and arboreal animals live in a cluttered environment, that is, one whose open spaces are encroached upon by solid surfaces. Nevertheless, they swim, fly, or take a course among these obstacles with great precision. Many species do so only when the medium is illuminated, that is, they depend on vision. The obstacles are specified in the optic array by contours with internal texture; the open spaces are specified by the areas between such contours. The background areas may be untextured homogeneous colour like the sky, or densely textured surface colour, like the earth. The question is, what governs the taking of a course during such locomotion? Symmetrical magnification of a textured contour specifies a collision course toward an obstacle, but a skewed magnification, where the centre of flow is outside the textured contour, specifies a non-collision course. So long as an animal keeps the focus of centrifugal flow outside the textured contours and within a homogeneous or densely textured area, he will not collide with a solid object. This formula, it may be noted, is simply the reciprocal of that for aiming at a goal object. The properties of a textured contour by which is specified either an obstacle on the one hand or a goal object on the other have not yet been considered.

(v) *Pursuit and flight*

Predatory animals pursue and preyed-upon animals flee. These biologically complementary forms of behaviour must both be controlled by transformations of optical pattern. Approach to a stationary goal object has been described. A fugitive goal object is specified when the optic array as a whole flows from a centre, but when a textured contour within it does not expand. The object is then moving away and being pursued. Absolute angular magnification of the contour means catching up and minification of the contour means falling behind. Hence, the rule by which a big fish can catch a little fish is

simple: amximize its optical size in the field of view. From the point of view of the prey, the expansion of a textured contour in the optic array means the approach of something. This in itself may touch off the reaction of flight if it comes within the field of view. Preyed-upon animals need eyes which can register this abstract optical event in the rearward as well as the frontward direction, and very generally they seem to have developed eyes with a panoramic binocular field (Walls, 1942). What exactly *is* flight? The formula is again simple. It is so moving as to minimize the optical size of the expanding contour. This will necessarily involve a centripetal flow of that hemisphere of the optic array which contains it and a centrifugal flow of the opposite hemisphere. If the optical size of the contour decreases, the animal is getting away; if it increases despite the contraction of the surrounding field, the animal is being overtaken. There is one other geometrical possibility of stimulation: contraction of a contour in an otherwise static array. This means something *going* away. Probably it does not touch off the reaction of flight as expansion of a contour does in preyed-upon animals. This hypothesis needs empirical testing.

(vi) *The complexities of terrestrial locomotion*

For simplicity, the formulae of the theory have referred, so far, to locomotion in a medium of water or air. They are sufficient to show the possibilities of a circular control or 'feedback' type of stimulus-response analysis for understanding the locomotor behaviour of fish and birds. There are, however, terrestrial animals. The surface of the earth is a substratum. Locomotion can occur, mechanically speaking, in two possible ways: by exerting force against the medium and avoiding contact with the substratum, or by exerting force against the substratum and keeping in more or less continuous contact with it. The latter type, terrestrial locomotion, seems to have developed later in evolution. Terrestrial animals utilize a great variety of mechanical procedures—crawling, gliding, hopping, walking, running and the like, but they all push against the ground in one manner or another. For these animals, touch is available to play some part in controlling locomotion—actually *change* of contact stimulation, or tactual motion. There is also classical muscle-joint kinaesthesia which, in these animals, is never discrepant with visual kinaesthesia as it may be for the fish or the bird in a flowing medium. Since these modes of controlling stimulation run parallel in terrestrial man, it has been easy for the human psychologist to overlook the visual mode. Only since man began to fly aircraft has the visual component in the guidance of locomotion forced itself on the attention of psychologists. And yet it was always there to be observed. Consider the perception of 'footing' in a man who is making his way over rough terrain. Or, better, consider the oculomotor co-ordinations of a cat, or of any animal which runs with 'due regard' to the footing. The visual aspect of the performance is just as precise as the muscular aspect. The animal has only the information supplied by the optical array to a moving point (or a pair of them) and yet it adjusts its behaviour to the convexities and concavities and other physical properties of the surface ahead of it. This consideration reminds one that animals respond to invariants as well as changes of stimulation—to the permanent properties of the environment as well as to their own motions in it. We should return to the problem of the exteroceptive function of vision and note whether the problem is reformulated after first considering its proprioceptive function. Animals are capable not only of locomotion in, but also of orientation to, their environment, that is, of object perception and even what psychologists have called 'space' perception.

IV. VISUAL ORIENTATION IN ANIMALS

Animals are not continually on the move; they sometimes remain at rest. Often the only movements that can then be observed are eye-movements. This means that the resting animal is exploring the optic array even if he is not exploring the environment. It is tempting to believe that the animal is still making some kind of implicit responses to the solid surfaces around him even if he is not explicitly poking his nose into things. He is certainly oriented to gravity and to the substratum when he maintains a constant posture. He is probably also oriented to the distant objects of his environment when he remains in the same place. In the fifth assumption, we may indeed find a basis for the latter kind of orientation. It seems to be possible for him to identify and react to the objects of his world by virtue of the optical properties of the light reflected from them to his eye. One might even wish to suggest that the animal, in his way, *perceives* the objects of his world.

Animals make different kinds of locomotor reactions to different objects. They approach food or shelter, they avoid obstacles, they pursue prey and they flee the predator. These are discriminative reactions and they require a different kind of stimulus-response theory than do the control reactions heretofore considered. We must now consider actions which are specific to those features of the optic array which do *not* change during locomotion rather than those which do. Such features of stimulation are not response produced and the responses are not circular. In such behaviour the S-R linkage is between permanent entities of the environment and acts which are appropriate to them. The distinction between an S-R theory of *control* reactions and an S-R theory of *identifying* reactions is important for behaviour theory. It is true that an automaton can be designed which will aim at, approach, and pursue a pre-set target (as witness military missiles) and that no automaton has yet been designed which will recognize targets appropriate to its own needs (apart from its designer's) and act accordingly. But it would be wrong to categorize the first kind of reactions as *automatic* and the second kind as *voluntary*. This dichotomy is as pernicious as the one between sensory and perceptual processes. The true distinction is probably that between the properties of stimulation which vary over time and those which do not.

(i) *Object perception*

By the fifth assumption, material objects, substances in the solid state, are specified by the textures and contours of the optic array, and they stop locomotion. However, a solid object, by virtue of its chemistry and biochemistry, may constitute food for an animal. Or it may constitute a mate, or young, or shelter, or an obstacle, or an enemy. These higher-order properties of solids are specified by higher-order properties of the light in the optic array. For example, there is internal pattern as well as texture in the light reflected from an object and there is shape as well as contour. There is a frequency distribution in such light which specifies in a complicated way what men call 'colour'. Moreover, if the object is alive and moves there will be deformations of its contour and internal pattern which specify it as animate, and which may further specify it as one or another kind of animate thing. In so far as an animal can discriminate these variables of the optic array, he can discriminate the properties of objects which render them not only bump-into-able and walk-on-able but also mate-with-able, or get-underneath-able, or edible, or likely to cause pain. And to this extent, he can identify the significant classes of objects

in his environment, that is, he can respond to them differently as the objects themselves differ in biophysical ways (Gibson & Gibson, 1955). These are the so-called 'goal-objects' of an animal's environment, the ones which are related to the animal's needs, which induce approach or avoidance, and which man describes as having valences or meanings in the perceptual field. The hypothesis is that the values of objects as well as their solidity are specified with some degree of reliability in the optic array. Mere physical solidity has a value for locomotion and collision. Chemical and biological properties of higher order have values for nutrition, reproduction, and survival. The animal's task is to detect these properties at a distance.

Contemporary behaviour theorists either take for granted that objects constitute stimuli ('stimulus objects') and thus beg the question of how objects are responded to, or else they assume vaguely that pattern stimuli are 'cues' for behaviour and thus slip around the problem of object perception. (The latter point is illustrated by comparing the use of 'cue' by a student of behaviour such as Miller—Miller & Dollard, 1941—who takes the concept very lightly, and a student of perception such as Brunswik, 1956, who takes the concept very seriously.) But this will not do. During locomotion, the patterns at the receptor surfaces of an animal are necessarily in flux, the objects being projected as changing patterns rather than constant patterns. If we wish to believe that an animal can respond to a constant object in the course of his behaviour we should first resolve this apparent contradiction. There are two alternatives. We can assume that the animal somehow constructs a perceptually constant object from the kaleidoscopic field of sensations: this is the starting point for classical theories of perception. Or we can assume that the animal is sensitive to the mathematically invariant properties in the stimulus flux which correspond to the physically constant object: this is the alternative which seems more promising for the future. During the locomotor transformations of the pattern of the optic array the property of colour remains, of course, invariant. During these perspective transformations many geometrical properties such as the kind of texture, or the angularity or curvature of the pattern also remain invariant. There are plenty of invariant variables in the flux of optical stimulation on the basis of which an animal can identify classes of permanent objects. The empirical question is whether animals are, or come to be, sensitive to them.

(ii) *Orientation to the visible environment*

Animals seem not only to recognize constant objects, but to orient themselves to a constant environment, that is, to any permanent arrangement of solid surfaces. The homing behaviour of many species suggests that the animal is able, on familiar ground, to find its way from any one place to any other significant place; territorial behaviour also suggests that he can respond, in some sense, to the habitat as a whole; and exploratory behaviour suggests a kind of striving to extend the boundaries of this whole. How can this complicated behaviour be supposed to depend on vision? Consider the pattern of an optic array to a stationary position. For land animals the lower hemisphere, roughly, will be textured and patterned by the surfaces and objects of the earth. But not only solidity is specified in the array; the slants and slopes and the facings of this arrangement of surfaces are specified by *gradients* of the optical texture (Gibson, 1950, chap. 6). The edges are specified by *steps* of the texture variables, and convexities or concavities are specified by *changes* of gradient. In short, the lay of the land, the jumping-off places, the

interspaces, barriers and obstacles, as well as the level stretches, are given by the geometry of the optic array. Depending on the locomotor capacities of the animal, this terrain provides definite possibilities or impossibilities for crawling, walking, climbing and the like. And if the animal can discriminate the textural variables it can discriminate among potential paths for locomotion. A potential path is a stretch of surface extending away from the animal which affords the kind of locomotion for which the animal is equipped. A barrier or obstacle is a surface which does not afford locomotion. There are transitional cases between path and barrier, to be sure, and a cliff or vertical edge is a special type of barrier, but the definitions will serve well enough for theoretical purposes. (The responses of rats to a 'visual cliff' have recently been studied under laboratory conditions—Walk, Gibson & Tighe, 1957—and they show the kind of aversion predicted by this theory.) A terrestrial animal is always encircled, then, by a radiating set of paths or barriers. Each angular sector of the optic array specifies the possibility of locomotion in that direction. A level path of unlimited distance is given by a gradual increase of textural density up the array to the horizon. A barrier is given by a region over which density remains constant, and its distance by the point at which the change in the density gradient begins. A falling-off place is given by an abrupt increase in density upward in the array. A margin between land and water (a barrier for a non-swimmer) is given by a change in the kind and rigidity of texture together with a change in colour. Confinement consists of barriers in all directions; freedom consists of paths in all directions. To the extent that the animal can respond to these variables of the encircling optic array, we may now conclude, he has locomotor orientation to his environment. He perceives the possibilities of locomotion surrounding him. *And this is probably what should be meant by asserting that an animal has visual space perception.*

(iii) *Orientation to the environment outside the range of vision*

The visible environment may be limited by enclosing surfaces such as those of a laboratory maze, and even in an open medium it is limited by the acuity of the eye, the clearness of the medium, and the level of the illumination. An animal, nevertheless, can learn in some degree to go to places and objects outside of these limits (Rabaud, 1928; Thorpe, 1956) just as a man can learn to find his way about a house, the streets of a city, or the highways of a state. How is this behaviour to be accounted for, and in what sense is it visually determined? It is not explained by the unchanging pattern of an optic array to a single position. Consider, however, the transformations of pattern of an optic array to a moving position, as described in the seventh and eighth postulates. We have assumed that a family of continuous transformations is itself an optical stimulus, and that the properties which remain invariant under transformations also remain stimuli during such flux. The former specifies the particular path of locomotion taken; the latter specify the permanent arrangement of surfaces. An animal that explores its environment in the course of time moves from each of many places to each of many other places and, just as each place corresponds to a unique pattern, so each movement corresponds to a unique transformation. The visual flux of a locomotor animal, as it goes about its business, consists of a sequence of transformation families interspersed with periods of non-transformation. Since these families each begin where the preceding one ended, they are linked. The entire life of the animal is co-extensive, in fact, with a single grand family of optical transformations which specify the history of its travels and explorations. This fact should

be taken into consideration in any theory of maze learning or, more generally, of how an animal learns its way about. The successive patterns of the optic array are not a series of discrete pictures like a series of nonsense syllables which have to be associated; they are parts of a continuous transformation with a temporal pattern of its own. The nature of this temporal pattern—the direction of the change of the momentary patterns—is probably something which an animal can sense and, if so, we do not have to postulate the conditioning of a series of responses to a series of discrete stimuli as Hull (1952) does in his theory of the goal-gradient and the habit-family. Hull chops a temporal pattern into a set of static patterns and is then faced with the problem of integration. The integration of behaviour is partly explainable in terms of the pre-existing integrity of stimulation, and by the hypothesis that an animal learns to respond to it. If maze learning is to be explained in S-R terms we must recognize that the stimuli *to which responses are made* change as well as the responses themselves (Gibson & Gibson, 1955). In the course of learning, a whole temporal pattern may become the effective stimulus for a single integrated act.

The controversy over 'response learning' and 'place learning' has now reached major proportions in psychology (Woodworth & Schlosberg, 1954, chap. 21; Thorpe, 1956, chap. 5), but neither side has troubled to analyse the optical stimulation. Hull's theory of maze learning asserts that it is the acquiring of a sequence of movements. Tolman's theory asserts that it is the acquiring of a cognitive map of the maze (Tolman, 1948). If Hull's theory could profit by utilizing the notion of continuous transformation, Tolman's theory could profit by the notion of invariant variables of stimulation under transformation. The perceiving of the possibilities of locomotion surrounding an animal does not, as Tolman assumes, require a theory of sign learning or the interpretation of sensory cues. Space perception can be explained with greater parsimony by kinds of stimulation hitherto neglected in sensory physiology. Knowing the possibilities of locomotion *outside* the limits of momentary vision, that is to say the cognitive mapping of the extended environment, can be explained in part by the recurrent, constant, or invariant properties of such stimulation which are discovered during exploratory behaviour.

(iv) *The learning of visual orientation by exploration*

Animals learn to get about the environment and they also learn *about* the environment. The first kind of learning is called behavioural and the second cognitive, but nothing prevents us from making both assertions. If an animal goes to where something is we can infer that he knows where it is. In either case the experimental evidence suggests that the learning depends on *exploratory* activity. Artificial environments such as mazes have provided much of the evidence, but the conclusion holds for natural environments as well, the evidence coming from work on homing and foraging (Thorpe, 1956). No other kind of stimulation is as informative about the environment as light when it is many-times reflected in a medium. The best means of exploring an environment is (a) to point the eyes in various directions, and (b) to plant them at various station points. It seems probable that eyes are sensitive to the transitions between visual patterns as well as the patterns themselves. Certainly locomotion is guided thus. A transformation may be more significant than a form, in fact, since it carries all the information that a form does plus that carried by the kind of transition. There are families and superfamilies of such information in focusable light. The more of it an animal can react to the more efficiently and

quickly he can go from place to place in his environment, and the more he can learn what the arrangement of his environment is. This kind of learning is by no means the only kind to be sure, but it is an important one.

REFERENCES

- BRUNSWIK, E. (1956). *Perception and the Representative Design of Psychological Experiments*. Berkeley: University of California Press.
- FRAENKEL, G. S. & GUNN, D. L. (1940). *The Orientation of Animals*. London: Oxford University Press.
- GIBSON, J. J. (1950). *The Perception of the Visual World*. Boston: Houghton-Mifflin.
- GIBSON, J. J. (1951). What is a form? *Psychol. Rev.* **58**, 403-12.
- GIBSON, J. J. (1954). The visual perception of objective motion and subjective movement. *Psychol. Rev.* **61**, 304-14.
- GIBSON, J. J. & DIBBLE, F. N. (1952). Exploratory experiments on the stimulus conditions for the perception of a visual surface. *J. Exp. Psychol.* **43**, 414-19.
- GIBSON, J. J. & GIBSON, E. J. (1955). Perceptual learning: differentiation or enrichment? *Psychol. Rev.* **62**, 32-41.
- GIBSON, J. J. & GIBSON, E. J. (1957). Continuous perspective transformations and the perception of rigid motion. *J. Exp. Psychol.* **54**, 129-38.
- GIBSON, J. J., OLUM, P. & ROSENBLATT, F. (1955). Parallax and perspective during aircraft landings. *Amer. J. Psychol.* **68**, 372-5.
- GRAHAM, C. H. (1951). Visual perception. Chap. 23 in S. S. Stevens (Ed.), *Handbook of Experimental Psychology*. New York: Wiley.
- HULL, C. L. (1938). The goal-gradient hypothesis applied to some 'fieldforce' problems in the behavior of young children. *Psychol. Rev.* **45**, 271-99.
- HULL, C. L. (1952). *A Behavior System*. New Haven: Yale University Press.
- LEWIN, K. (1933). Environmental forces. Chap. 14 in C. Murchison (Ed.), *A Handbook of Child Psychology*. Worcester: Clark University Press.
- LEWIN, K. (1935). *A Dynamic Theory of Personality*. New York: McGraw-Hill.
- MILLER, N. E. & DOLLARD, J. (1941). *Social Learning and Imitation*. New Haven: Yale University Press.
- NICOD, J. (1930). *Foundations of Geometry and Induction*. London: Kegan Paul, Trench, Trubner and Co.
- RABAUD, E. (1928). *How Animals Find their Way About*. New York: Harcourt, Brace.
- SHERRINGTON, C. S. (1906). On the proprioceptive system especially in its reflex aspect. *Brain*, **29**, 467-82.
- THORPE, W. H. (1956). *Learning and Instinct in Animals*. London: Methuen.
- TINBERGEN, N. (1951). *The Study of Instinct*. London: Oxford University Press.
- TOLMAN, E. C. (1948). Cognitive maps in rats and men. *Psychol. Rev.* **55**, 189-208.
- WALK, R. D., GIBSON, E. J. & TIGHE, T. J. (1957). Behavior of light- and dark-reared rats on a visual cliff. *Science*, **126**, 80-1.
- WALLS, G. L. (1942). *The Vertebrate Eye*. Bloomfield Hills, Mich.: Cranbrook Institute of Science.
- WOODWORTH, R. S. & SCHLOSBERG, H. (1954). *Experimental Psychology*. London: Methuen.

(Manuscript received 28 June 1957)

EXPERIMENTS ON 'NEOPHOBIA' IN WILD AND LABORATORY RATS

By S. A. BARNETT

Department of Zoology, University of Glasgow

All rats (*Rattus norvegicus*), wild or tame, have a well-marked tendency to explore their surroundings. Wild rats, but not tame ones, also tend to avoid unfamiliar objects in a previously explored, and therefore familiar, area. The effect of environmental change on feeding was studied in three groups of wild rats. Rats of Group 1 were accustomed to feeding from a wire basket at the back of the cage; for them the change consisted of transfer of the food to an unfamiliar tin at the front of the cage. Group 2 had food in the basket throughout, and the change consisted only in an empty, unfamiliar tin being put in the front of the cage. The rats of Group 3 fed from a basket in the front of the cage; the change for them was the placing of an unfamiliar empty tin between the back of the cage, where they slept, and the food. In all three groups feeding was interrupted by the change. In the first group, three of the five rats stopped eating completely for one or more days; in the other two groups, food consumption was merely reduced on the first day after the change. The check in eating was not due to exploration: the new object was at first completely avoided by all the rats. Tame albino and hooded rats, subjected to the same procedure as the wild rats of Group 1, investigated the tin and the food in it at once; their food intake during the day after the change was unaffected. The first approach of the hooded rats to the new object was, however, more hesitant than that of the albinos. Investigation of a new object, or new conditions, can sometimes lead to an interruption of feeding in tame rats, but this has nothing in common with avoidance behaviour in wild rats.

I. INTRODUCTION

During the movements of wild rats about their living space, two apparently opposed types of response may be observed. The first is exploration: this is especially clearly seen when a new area becomes accessible (Barnett & Spencer, 1951), or when rats are experimentally introduced into an unfamiliar cage or enclosure (Barnett, 1958*a*); this sort of behaviour is highly developed in tame laboratory rats as well as in wild ones. The second type of response is the avoidance of unfamiliar objects in familiar surroundings (Chitty & Shorten, 1946); this 'neophobic' behaviour, as will be further shown below, does not occur in typical laboratory-bred rats. The experiments described below were designed to clarify the relation between exploration and avoidance, with special reference to the differences between wild and tame rats. A general discussion of exploratory and allied behaviour will appear separately (Barnett, 1958*b*).

II. EXPERIMENTAL

The rats used were healthy, adult, virgin females, born in the laboratory. The wild rats were aged 63–131 days at the beginning of the experiments in which they were used; their initial weights were 115–205 g. The tame rats were of two types, neither rigorously inbred: they were (i) albinos, aged 66 days and weighing 132–157 g., and (ii) hooded rats, aged about 70 days and weighing 150–190 g. Unlike the wild rats, they could, as usual, be readily handled without special precautions. Each rat was kept alone in a wire cage measuring 36 by 25 by 16 cm. high, and had been in the cage at least 7 days before observations began. The floor of each cage was raised; 5.5 cm. below it was a tray containing peat moss covered with thick paper; food residues fell on this paper and were collected. The weight of food eaten by each rat was recorded daily. Both before and during the experiments the only food supplied

was diet 41 (Bruce & Parkes, 1949). Excess water was always available, supplied from an inverted bottle on top of the cage.

To illustrate the effect of changed conditions on food consumption, three types of experiment were carried out. In the first, the food was initially supplied in the usual manner, in a wire basket suspended from the cage roof; this basket, and the water source, were at the back of the cage, that is, the part of the cage near the wall and farthest from people (Fig. 1); this was where the rat usually slept. When the rat was fully accustomed to this arrangement, its daily food consumption was recorded for 4 days. On the fourth day, a tin was put in the front of the cage, fixed to the wire, and food was supplied in this tin; the basket in which food had hitherto been supplied was left empty. This type of experiment was carried out with five wild rats, five albinos, and five hooded rats.

In the second type of experiment the procedure was the same except that (a) food was supplied in the basket throughout, and the tin was left empty, and (b) the change was made after only 3 days. In this case, then, the only change produced was the introduction of an unfamiliar object. This type of experiment was carried out with five wild rats.

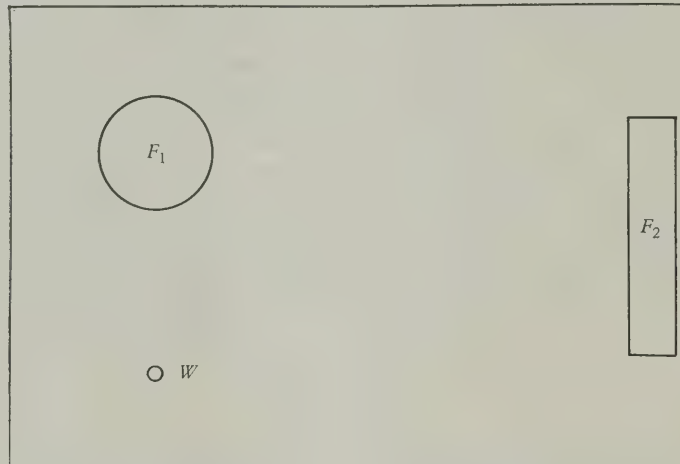


Fig. 1. Arrangement of cage for experiments of types 1 and 2. F_1 = food basket; W = water source; F_2 = food tin. Back of cage on left.

In the third type of experiment, the food basket was at the front of the cage instead of the back while the water source was still at the back. Consequently, in order to feed, the rats had to come forward from where they usually slept. After 3 days, an empty tin was put in each cage (Fig. 2), so that this unfamiliar object had to be passed when the rat approached the food. Food was supplied in the basket throughout, never in the tin. This type of experiment was carried out on six wild rats.

The room in which the experiments were done contained many other rats. Its temperature was kept between 19 and 23° C., and it was continuously ventilated by an extractor fan. There is no reason to think that there was any substantial variation in the olfactory stimuli to which the rats were subjected during the experiments.

III. RESULTS

The most drastic alteration of conditions was that imposed in the experiments of the first type, in which the food was transferred from a basket at the back of the cage to a completely unfamiliar tin at the front. In the first phase of this experiment, before the changeover was made, the wild rats always began to eat within 3 hr. of the replenishing of the food baskets (at about 11.00 hr. G.M.T.); this was shown by the presence of kibble fragments below the cage floor. After the change on the fourth day, no rat had fed at all by 18.00 hr., that is, 7 hr. after the new arrangement had been made. All dropping

were at the back of the cage, showing that the rats had remained there and that none had investigated the new object or had made any general search for food.

The daily intake of food is shown in Fig. 3; in this case, in view of the variation between individual rats, the record is given separately for each. Despite the altered behaviour of all rats during the first 7 hr., two of the rats (nos. 4 and 5) showed only a slight decline in food consumption on the fifth day. The other three ate no food at all on that day; of these, no. 3 began to eat on the seventh day, no. 1 on the eighth day, and no. 2 on the ninth. The position of kibbled remnants below the cage floor showed that all eating was done, by all the rats, at the back of the cage. Further, once they had begun to feed, all the rats developed the habit of removing the food cubes from the tin to the back of the



Fig. 2. Arrangement of cage for experiment of type 3. Lettering as Fig. 1. Back of cage on left.

cage. On the tenth day, all food had been removed from all tins, except for four cubes left in the tin by rat no. 5. At the end of the experiment, proper food was once more put in the basket, as well as in the tin; but the rats continued to remove the cubes from the tin and to collect them at the back of the cage.

The effects of the less drastic changes in the experiments of the second and third types are shown in Figs. 4 and 5. In the experiments of the second type, all the rats showed some reduction in food consumption during the 24 hr. after the introduction of the tin, but all ate some food. The mean consumption on day 4 is significantly different from the mean consumption on day 3 ($P \leq 0.05$). Judged by the absence of droppings in the front of the cage, the tin was entirely avoided during this 24 hr. period: the rats evidently remained at the back of the cage and refrained from exploration. In subsequent days, droppings were seen in front of the cage; but no detailed observations were made on the stages by which the 'shyness' of the new object was overcome.

In the experiments of the third type, one of the rats ate the same weight of food on the fourth day as on the third, but the rest all ate less; the mean consumption of food on day 4 was however not significantly different from that on day 3 (Fig. 5). Nevertheless, as in the other experiments, there was a delay in visiting the front of the cage: there was no evidence of feeding or of exploration during the first 5 hr. after the change had been

made. In this situation the rats had to go to the front to feed, since the construction of the basket precluded carriage of food cubes to the back.

Finally, the response of the tame rats, to the first type of experimental procedure must be described. Within 4 min. from the time when the unfamiliar tins, containing food, had been placed in the cage, all the albinos were investigating the tins and removing cubes; their approach was made without hesitation. Fig. 6 shows that daily food intake was not appreciably influenced by the change of conditions. One rat ate only 11 g. or

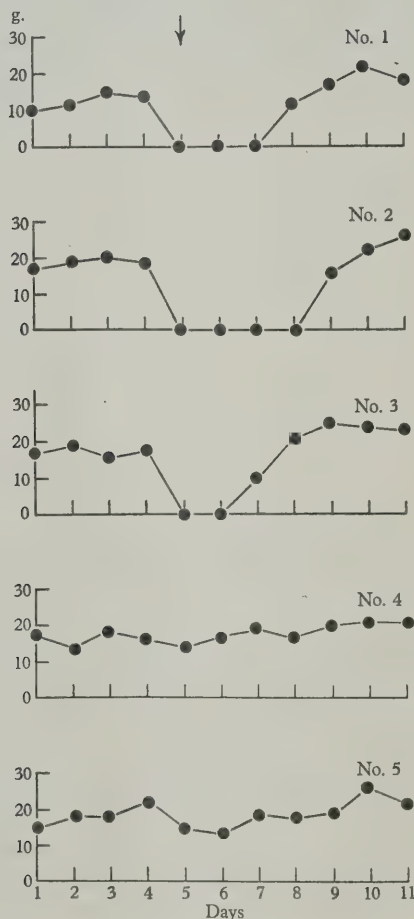


Fig. 3. Daily food consumption in g. of individual wild rats subjected to experimental procedure, type 1.

the fourth day, after eating 14, 15 and 16 g. respectively on the previous days, followed by 17 g. on the fifth day. One rat ate 3 g. more on the fourth day than on the third; the consumption of two others differed by only 1 g. from that of the day before; and that of one was unchanged. The mean food consumption on day 4 does not differ significantly from that on day 3. All rats, from the first day on which they had the opportunity regularly removed the cubes from the tin, and scattered them about the cage.

The hooded rats also investigated the tins almost at once: four had begun to do so by

the seventh minute, but one delayed until the twentieth minute. By the fifteenth minute the first four were also removing food cubes. Fig. 7 shows that, as with the albinos, daily food intake was not materially altered. The manner of approach of these rats to the food tin differed, however, from that of the albinos: the initial movements were more tentative and hesitant; in this, they resembled those of a wild rat faced with a new object, but the hesitation was much sooner overcome. Four of these rats scattered food about the cage, but all ate usually at the back.

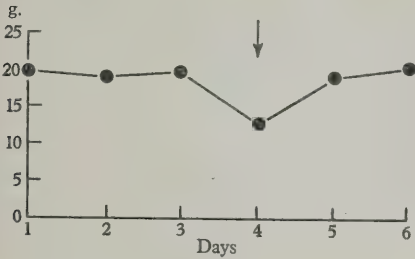


Fig. 4

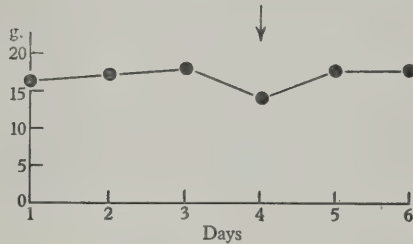


Fig. 5

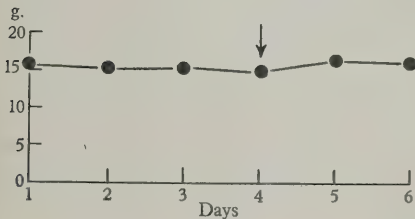


Fig. 6

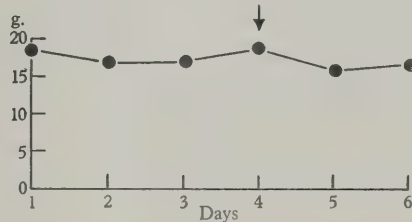


Fig. 7

- Fig. 4. Mean daily food consumption in g. of five wild rats subjected to experimental procedure, type 2.
 Fig. 5. Mean daily food consumption in g. of six wild rats subjected to experimental procedure, type 3.
 Fig. 6. Mean daily food consumption in g. of five albino rats subjected to experimental procedure, type 1.
 Fig. 7. Mean daily food consumption in g. of five hooded rats subjected to experimental procedure, type 1.

IV. DISCUSSION

The results of all three experimental procedures show that what Chitty & Shorten (1946) call 'new object reaction' can be displayed even in a small cage. This neophobia consists of avoiding an unfamiliar object in familiar surroundings (and also, in some instances, a considerable area around it). Such a response must clearly tend temporarily to reduce food intake when accustomed food is replaced by a new food (Chitty & Shorten 1946; Chitty, 1954; Shorten, 1954), although Shorten (1954) distinguishes between new object reaction and the refusal of an unfamiliar foodstuff (cf. Barnett, 1956). The placing of a new object near or at a rat's usual feeding place has a similar effect: the behaviour involved has been extensively discussed both by Chitty and by Shorten in the papers quoted.

In the first experimental procedure the change in conditions was complex; it included the introduction of a new object, and the transfer of the food to this object. The *absence* of food from the basket which had hitherto contained it was probably unimportant (Shorten, 1954), except that it obliged the rats to get their food in an unfamiliar place.

The effect of the behaviour of three of the five wild rats, to which this procedure was applied, was drastic. The contrastingly brief interference it caused in the feeding of the other two rats of this group reflects the great individual variation which must be expected in dealing with members of a genetically heterogeneous population. This variation in behaviour has also been recorded by Chitty (1954) from observations on free-living colonies. Had food intake been recorded at shorter intervals than 24 hr., the interruption in the feeding of all five rats could have been more clearly shown.

The environmental changes imposed in the other experiments were relatively trivial. In the third type of experiment, the introduction of a new object between a rat and its food merely inhibited food consumption during the first 5 hr. of the first day, evidently by delaying the approach to the already familiar food and container. Qualitatively, this may be regarded as an example of typical new object reaction. The results of the second type of experiment were similar. Here the new object was put in the front of the cage, but the rats were still able to feed as usual at the back. Nevertheless, food intake was significantly reduced on the first day. The reduction in food eaten was in neither case due to preoccupation with exploration or investigation of the new object: on the contrary, the new object was avoided at first. Thus 'new object reaction' in these situations was clearly a result of 'fear' induced by the introduction of the unfamiliar object, presumably visually perceived, into the immediate environment of the rat. Montgomery & Monkman (1955) have shown that, in laboratory rats too, 'fear' reduces exploration.

The result of applying the first type of experiment to albino rats illustrates the complete absence of avoidance behaviour or new object reaction in any form in these animals. The response to new conditions is an immediate and vigorous exploration. There was no alteration in the daily rate of food consumption. Much the same applies to hooded rats, but in them the first approach to the new object was more wary; it is possible that this is a consequence of their superior vision since, unlike albinos, hooded rats have normal pigmentation of the iris. As already mentioned, exploration is also a conspicuous feature of the behaviour of wild rats (Barnett, 1956; see also Barnett & Spencer, 1951; Barnett, 1958*a*); but it is inhibited in the special conditions of the introduction of a new object into familiar surroundings.

The results of the experiments given above show, even without the observations of other workers, that the interference with food consumption resulting from unfamiliar stimuli is exceedingly complex. Among the processes which may be involved are (*a*) the failure to visit a place in which food has been newly placed, and (*b*) the positive avoidance of a food which, in an unfamiliar position, constitutes a 'new object'; but positive avoidance is undoubtedly the more important of these two, since, in the absence of some specific source of inhibition, every part of a rat's home range is probably visited regularly (Barnett, 1956, 1958*a*).

The use of food consumption as an index of 'new object reaction' can lead to further difficulties of interpretation. Hebb (1949, p. 194), Campbell & Sheffield (1953) and Berlyne (1955) have described how changes of environment induce exploration in laboratory rats. Chance & Mead (1955) draw attention to the fact that reduced food intake in a new situation may result either from delay in beginning to eat or from a lower rate of feeding, or both. Their experiments, which illustrate this difference, were on albino rats; consequently, in them, any interference with feeding due to novel stimuli must be attributed to the priority given by tame rats to exploration rather than eating, as first described

by Small in 1899. Chance & Mead, however, equate the effect they observed with the new object reaction described by Chitty & Shorten in wild rats. This is clearly mistaken. The experiments described above show: (i) that exploration on the one hand, and new object reaction on the other, are quite distinct; (ii) that only wild rats display new object reaction in any substantial form; and (iii) that in wild rats avoidance behaviour is by far the more important source of delayed or reduced feeding.

Thanks are due to John Keys and Margaret MacLean for technical help.

REFERENCES

- BARNETT, S. A. (1956). Behaviour components in the feeding of wild and laboratory rats. *Behaviour*, **9**, 24-43.
- BARNETT, S. A. (1958*a*). An analysis of social behaviour in wild rats. *Proc. zool. Soc. Lond.* **130**, 107-52.
- BARNETT, S. A. (1958*b*). Exploratory behaviour. *Brit. J. Psychol.* (in the Press).
- BARNETT, S. A. & SPENCER, M. M. (1951). Feeding, social behaviour and inter-specific competition in wild rats. *Behaviour*, **3**, 229-42.
- BERLYNE, D. E. (1955). The arousal and satiation of perceptual curiosity in the rat. *J. Comp. Physiol. Psychol.* **48**, 238-46.
- BRUCE, H. M. & PARKES, A. S. (1949). Feeding and breeding of laboratory animals. 9. A complete cubed diet for mice and rats. *J. Hyg., Camb.*, **47**, 202-8.
- CAMPBELL, B. A. & SHEFFIELD, F. D. (1953). Relation of random activity to food deprivation. *J. Comp. Physiol. Psychol.* **46**, 320-2.
- CHANCE, M. R. A. & MEAD, A. P. (1955). Competition between feeding and investigation in the rat. *Behaviour*, **8**, 174-82.
- CHITTY, D. (1954). *The Control of Rats and Mice*. Vol. I. *Rats*. London: Oxford University Press.
- CHITTY, D. & SHORTEN, M. (1946). Techniques for the study of the Norway rat (*Rattus norvegicus*). *J. Mammal.* **27**, 63-78.
- HEBB, D. O. (1949). *The Organization of Behavior*. New York: Wiley.
- MONTGOMERY, K. C. & MONKMAN, J. A. (1955). The relation between fear and exploratory behavior. *J. Comp. Physiol. Psychol.* **48**, 132-6.
- SHORTEN, M. (1954). In *The Control of Rats and Mice*. Vol. II, Chitty, D. (ed.). London: Oxford University Press.
- SMALL, W. S. (1899). Notes on the psychic development of the young white rat. *Amer. J. Psychol.* **11**, 80-100.

(Manuscript received 16 April 1957)

THE ROLE OF THE REINFORCER AS A STIMULUS

By R. L. REID

Psychology Department, University of Aberdeen

The reinforcing event is present as a stimulus in the situation in which a response is acquired and is absent from the extinction situation. When extinction conditions are made more similar to those of acquisition by introducing the reinforcer unrelated to behaviour ('free' reinforcement), the extinguished response is restored. Experiments with students, pigeons, and rats are described; and an analysis of the stimulus effects of reinforcement is used to account for 'disinhibition', 'spontaneous recovery', and speedy reconditioning after extinction.

I. INTRODUCTION

Reinforcing events are sources of stimulation, whatever else they may be, and therefore, in learning situations, they may be expected to gain some degree of stimulus control over the performance of the learned response. The experiments* that are described below demonstrate that renewed responding tends to occur when an extinction situation is made more similar to an original acquisition situation by adding the reinforcing event as a stimulus.

II. EXPERIMENTS 1-4

1. *Restoration of extinguished operant in rats*

A conventional Skinner box was used. The floor space was 10 by 8 in. The manipulandum, a metal bar, was set at a height of 2 in. and the food cup was placed at floor level below the lever and slightly to the right. Responses were recorded by a Harvard Cumulative Recorder. (This recording instrument—used also in Exps. 2 and 3—was provided through a grant from the American Academy of Arts and Sciences.)

Six rats, deprived of food for 8 hr., were reinforced 20 times on each of 2 successive days of training by the delivery of one small food pellet following all bar-pressing responses. On the next 3 days each rat was put into the box for 30 min. extinction, lever-pressing now producing the usual noises but no pellets of food.

At the end of the last extinction period, when the frequency of responding was minimal, a number of stimuli were presented. They included tapping the box, flickering the lighting and sounding a buzzer. These stimuli had no detectable 'disinhibitory' effect. A few minutes later, a pellet of food was dropped into the food dish. All rats ate immediately and five out of six pressed the bar at least once during an observation period of 1 min. following eating. No rat had responded during the equivalent observation period before the delivery of food. Thus the 'free' issue of food had the effect of restoring the extinguished response.

Skinner (1938) mentions a similar case where the discharge of three pellets of food from the magazine by the experimenter resulted in some restoration of responding in an extinguished rat. He also reports a failure to obtain 'disinhibitory' restoration of operant responses when other stimuli were used.

2. *Restoration of extinguished operant in pigeons*

Six pigeons maintained on limited rations were trained to peck at a target set on the front panel of a Skinner box at a height of 8 in. The floor space of the box was 12 by 14 in. As reinforcement, one maple pea was delivered into a food dish at floor level below the pecking target for each correct response.

* Exps. 1, 2, 5(a) and 5(b) were part of an investigation supported by a research grant from the University of New Zealand.

After preliminary training with a lure, the pecking response was reinforced 50 times on each of 2 successive days. The response was then extinguished in three daily periods of 30 min. with the apparatus set as before, except that the food magazine was now empty.

At the end of the second extinction period, a number of tests of 'disinhibition' were made with a variety of stimuli. One of these, the rattling of maple peas, had a slight effect on the extinguished response in the case of two of the birds. The others had no effect. At the end of the third extinction period, one maple pea was dropped into the food pan at a prearranged time. All birds ate immediately, and five out of six pecked at the target within 10 sec. of eating. The bird that did not peck was given a second trial and responded then. No responses occurred in the equivalent periods before the free issue of food.

Thus rats and pigeons behave similarly when given 'free' reinforcement under similar conditions. In the case of pigeons, one stimulus (rattling of peas) may be slightly effective as a 'disinhibitor'. This is discussed later.

3. Restoration of extinguished operant in students

A slot machine was used. Each student was given forty counters and asked to aim at having the greatest possible number of counters in his possession at the end of the experiment. He was told that he could insert counters in the machine or not, just as he pleased, and that if he became bored he could read instead. Some old dry 'weeklies' were at hand.

Subjects of one group received total reinforcement. The first thirty-two responses of inserting a counter delivered two counters per response. Subjects of the other group were given irregular partial reinforcement. Of the first thirty-two responses, sixteen were reinforced by the delivery of four counters.

Extinction followed reinforcement without a break and continued until the subject had reached the criterion of making no responses during a period of $1\frac{1}{2}$ min. A test of the effect of 'free' reinforcement then began. A control observation period of 30 sec. was followed by a 'free' delivery of two or four counters and a further period of 30 sec. observation. Of thirteen subjects, two responded during the control period while twelve responded in the test period immediately after the 'free' reinforcement. The partially reinforced subjects generally produced bursts of responses.

All of the subjects who responded acknowledged a connexion between their response and the 'free' issue of counters. Asked what this connexion was, they gave answers that fell into three groups. Four provided a rationale of some sort, for example, that the free issue was intended to give a hint or that it indicated that the machine had been refilled. Two explanations of this type were preceded by the qualifying phrase, 'I suppose I thought...'. Four subjects gave 'reasons' such as 'Well, I had some in my hand so I thought I would try again' or 'I thought I might as well, when I was given two more'. The remaining four fell into the 'don't know' category. On the whole, there was little to suggest that the behaviour was the outcome of reasoning in the normal sense of the word.

4. Restoration of extinguished respondent in students

Similar restorative effects can occur when the u.s. is presented alone after the extinction of conditioned respondent behaviour. Pavlov (1927) reported some observations by his collaborators using the response of salivation in dogs. An experiment carried out by Brebner (1957) at Aberdeen shows that the effect occurs with a different species and a different response.

In student subjects an eyelid response was conditioned to a tone of 1110 cyc./sec. Reinforcement was given on 60% of the acquisition trials using a puff of air to the cornea as u.s. After each subject had reached the criterion by making four c.r.'s on four successive test trials, the u.s. (puff of air) was entirely discontinued and extinction trials were given until four successive failures to respond to the tone had occurred. The u.s. was then presented alone on one occasion. Subsequently, when the c.s. was presented alone seven out of fourteen subjects responded. Thus, the restorative effect of 'free' reinforcement occurred as in operant conditioning although less markedly.

III. INTERIM DISCUSSION

The experiments indicate that the restorative action of 'free' reinforcement is of some generality. In assessing the magnitude of the effect it should be remembered that 'free' reinforcement will strengthen whatever behaviour it happens to follow and, since this

behaviour is something other than the performance of the learned response, some counter-action of the restorative effect may have occurred.

The experiments might be regarded as demonstrating a type of 'disinhibition', but the theory that this term suggests may not be necessary and some additional hypothesis would still be required to account for the special effectiveness of 'free' reinforcement compared with other stimuli, as in Exps. 1 and 2.

As a first step towards explanation, the following empirical generalization may be useful: 'Maximal responding during extinction is obtained only when the conditions under which the response was reinforced are precisely reproduced' (Skinner, 1938). During extinction, the reinforcing event is necessarily absent. Considered as a stimulus it can be expected to have gained considerable control over the performance of the learned response, so that renewed responding will occur when it is restored to the extinction situation as a stimulus.

The principle provides a general framework for an account of the observations, but it leaves gaps to be filled in by a more detailed examination of the stimulus effects of reinforcers. For example, the stimulus of 'free' food may initiate a chain of activities with a corresponding chain of stimulation. The extinguished rat may eat the food, clean his whiskers, move around in the box and look at the bar. Giving free food may have quite remote stimulus effects, exteroceptive as well as interoceptive.

Some further experiments with pigeons may help to show what goes on. They deal with the restoration of responses originally set up by generalization rather than direct reinforcement. One procedure shows the effect in a very persistent form.

IV. EXPERIMENTS 5(a) AND 5(b)

5(a) Restoration of extinguished generalization in pigeons

The subjects were thirteen birds from an experiment on sensory preconditioning that had given negative results (Reid, 1952). Birds from both control and experimental groups were used. No difference in the behaviour of the different groups was observed either in the original experiment or in the secondary experiment that is reported here.

The birds were trained in a Skinner box (floor space 12 by 12 in.) to respond to one signal and not to another. A buzzer and a light stimulus each of $1\frac{1}{2}$ sec. duration were used. The response of pecking at a target 8 in. from floor level was reinforced by the delivery of one maple pea every time it occurred within 5 sec. of the onset of the positive stimulus. Trials were given at slightly irregular intervals averaging $1\frac{1}{2}$ min., and only one response per trial was reinforced. (During training, reinforced responses commonly took place just after the stimulus had been presented rather than during the brief period of presentation.) Generalization to the negative stimulus was extinguished in two experimental periods. On a final test, the negative stimulus was presented five times and responses were recorded during periods of 5 sec. preceding and following its presentation. One bird responded before the stimulus on one occasion and three birds made single responses after. The effect of the five stimulus presentations can be indicated by a score of 2 (responses after, minus responses before).

On the next fifteen trials, the negative stimulus was presented for 1 sec. and simultaneously a maple pea was delivered into the food dish at floor level below the pecking target. The effect of the negative stimulus was then retested on five trials when it was presented alone as before. Again, one bird responded once before the stimulus, but eleven birds now responded after, making a total of thirty-four responses out of a possible fifty. As a result of the fifteen trials of 'free' reinforcement, the score indicating the effect of the negative stimulus increased from two to thirty-three.

During the 'free' reinforcement trials, two birds were observed to peck at the target *before* eating the food. They were excluded from the results quoted above, but their behaviour is worth considering. To the observer, the beginning phase of the chain of behaviour set off by the 'free' food is similar to the beginning of the pecking response. During training, birds normally peck at the target from a position directly in

front of it. The food that is delivered as reinforcement is picked up and swallowed and the bird moves away. Between stimuli there may be ritualistic pacing or turning in the cage or the bird may settle down quietly in one part. The onset of the stimulus generally occurs when the bird is not in the pecking position and its first effects are to produce orientation followed by movement into this position. When 'free' food is delivered into the dish the first effects are highly similar. The bird orients and moves forward in the cage. In the case of the two birds under discussion, these preliminaries may have been so strongly connected with the movement of pecking at the target that this movement became pre-potent even over eating. In other birds, occasional compromise responses were noticed, e.g. tapping lightly on the wall of the box between the target and the food dish.

During the test trials from which the main result of the experiment was obtained the negative stimulus was presented alone. Under these conditions, the stimulus sets off orientation and forward movement through its association with the delivery of 'free' food. Since no food is present during the test trials, the pecking response has no strong competitor. However, two birds that did not respond to the stimulus during the test showed vigorous incompatible behaviour of pecking at the empty food dish.

The interpretation of the main result of the experiment in terms of the chaining of responses is consistent with a failure to obtain the effect when the free food was delivered to different parts of the cage at random rather than to the food dish. Under those conditions, the negative stimulus on test trials produced a type of searching behaviour.

An alternative explanation of the effect of 'free' reinforcement should be mentioned, if only to be dismissed. During the fifteen trials of food delivery some birds were observed occasionally to peck at the target immediately after eating. It is quite probable that this pecking may at times have occurred when the maple pea was still moving down the pigeon's lengthy oesophagus. A need-reduction theorist might believe that direct reinforcement of the connexion between the negative stimulus and pecking could occur under these circumstances. However, this type of forward action of reward seems unlikely, since the behaviour of pecking after free food was never very frequent and died out in all cases before the test started. The number of pecks made by individuals on test trials was uncorrelated with the number of pecks that had occurred after 'free' food.

Pecking after eating food delivered as reinforcement usually occurs at the beginning of discrimination training as a generalization effect. When training is done with a lure and care is taken to avoid following this behaviour with food it dies out in about fifteen trials, showing that on balance it is not being reinforced by the eating that immediately precedes it. The revival of pecking after eating that occurs when 'free' food is given can be accounted for by an analysis of conditions of stimulation, but detailed consideration of the point would not be in place here.

5(b) *Restoration of extinguished generalization—continuous test*

Fifteen trials of 'free' reinforcement were given in the last experiment, but the figure is arbitrary and the restorative effect could be expected to reach a maximum with some unknown number. If the interpretation that has been suggested is correct, too few trials would not produce a strong enough conditioned approach movement to the region of the target and food dish, while too many would result in strengthening the direct movement to the food dish so strongly that it would completely suppress the incompatible response of pecking at the target. The final experiment tries to circumvent this difficulty. From an anthropomorphic point of view, the procedure can be regarded as an attempt at muddling the birds as much as possible. (Animals may often have been confused by experimental procedures but rarely by intention.)

The apparatus and stimuli used were similar to those of Exp. 5(a). Discrimination training was given to six birds until, on forty criterion trials, they responded to all twenty presentations of the positive stimulus within 5 sec., but did not respond more than once to twenty equivalent presentations of the negative (generalization) stimulus. (Three birds were tested with both stimuli arranged in an irregular sequence, and three were tested with the positive and negative stimuli presented in groups, but there are no obvious differences in their later behaviour.)

The response of pecking after 'free' food was eliminated in fifteen trials when food was given uncorrelated with any stimulus. The confusing procedure then began. In an irregular sequence the birds were presented with: (1) the positive stimulus (response reinforced), (2) the negative stimulus plus a free delivery of food, and (3) the negative stimulus alone (response unreinforced). Fifty presentations of each were given at the rate of 10 per day, the interval between each presentation varying irregularly around 1½ min.

Scores were calculated for each day's performance. They indicate the effect of the negative stimulus when presented alone. They are obtained as before by subtracting the number of responses made in the five-second periods preceding the stimulus from the number of responses made in the equivalent periods following the stimulus. Before the introduction of condition (2), i.e. before the test started, the average score for ten trials of the negative stimulus was 0.33. On the first ten trials of the new procedure it rose rapidly and was maintained throughout the whole of the period of testing with little decrement (Fig. 1). For the whole of the 50 trials the average score was 26.3 (27.6-1.3). Unfortunately, the experiment had to be broken off at the 50th test trial, but it is clear that the response to the negative stimulus produced by this type of 'free' reinforcement procedure was highly persistent. The response was not accompanied by a general breakdown of discrimination, pecking before the onset of the stimulus remaining minimal.

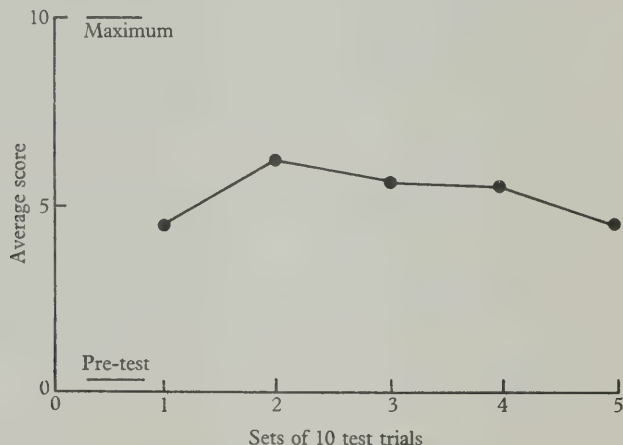


Fig. 1. Average scores for six birds on five sets of ten test trials. Horizontal bars indicate pre-test and maximum scores.

The confusing procedure ensures that reinforced pecking and eating are part of the bird's immediate past behaviour and that movement forward in the box in response to the negative stimulus is maintained in strength by the delivery of 'free' food in conjunction with this stimulus. Movement forward is an early part of a chain of behaviour that may carry through to pecking at the target when the stimulus of freely accessible food is not present to divert it. One bird was discarded for pecking at the target before eating 'free' food.

V. DISCUSSION AND CONCLUSIONS

The results of these experiments can be understood through an analysis of the stimulus situations in which responses are made. The term 'set' might have been used in a descriptive way but there seems to be no need to isolate any group of stimuli as a class with special behavioural effects. The experiments are not put forward in support of any theory of reinforcement or extinction, but rather to suggest that when situations are closely examined, the need for theorizing may be reduced. Inhibition theories of extinction are widely held because the effects of extinction procedures do not appear to erase the effects of original learning. Demonstrations of 'disinhibition', speedy reconditioning after extinction, and 'spontaneous recovery' are taken to indicate that extinguished responses are somehow 'there all the time but inhibited'. It may not be necessary to think in these terms.

(1) '*Disinhibition*'

What is validly recorded for some purposes in a learning experiment as a single response may be part of a chain of behaviour. Chaining is most obvious in free operant behaviour, where, for example, a rat may press a lever, eat and press again repetitively. It is not surprising to find that Skinner (1938) discusses the matter in some detail. The regularity of operant responding may be partly attributed to the shortness of the cycles of behaviour that occur. In Pavlovian conditioning, chaining is less obvious because the stimuli are presented intermittently at long time intervals and bodily movement is restricted physically in the case of animals and by instructions in the case of human subjects. The occurrence of the C.R. may, however, be strongly influenced by the animal's state of vigilance and by its maintenance of posture (Guthrie, 1952; Razran, 1956). Although the experimenter records nothing between trials, this does not mean that the C.S. breaks in on a stimulus vacuum of any sort.

Pavlov found that in states of acute extinction the introduction of an extra stimulus would lead to renewed responding, provided that the intensity of the stimulus was not high enough to cause a strong 'defensive orientation reflex'. This is not surprising in view of the unfailing soporific effects of Pavlovian extinction. Any stimulus that revives vigilance and the posture that goes with it without emotional disturbance will restore some of the normal contextual stimuli of the C.S., so that when it is next presented a response can be expected. Since sleep was regarded by Pavlov (1927) as '... nothing but internal inhibition which is widely irradiated...', any stimulus that is generally arousing could correctly be called a 'disinhibitor', but Pavlov seems to have envisaged a much more specific type of action. In the light of the present analysis it is understandable that the most effective 'disinhibiting' stimuli are those that make the dog salivate, especially the original U.S. Once started, salivation generally continues for a few minutes and may have much more lasting effects. For example, Pavlov reports an experiment on trace conditioning in which the U.S. is regularly presented to a dog every 30 min. If one presentation is missed the dog salivates at, or slightly later than, the 30th minute from the last presentation. The occurrence of the U.S. and U.R. at a given time can thus be registered and affect behaviour much later. When 'free' reinforcement is given in an extinction situation, the normal context in which the C.R. was established is most fully restored. The importance of the response as a source of stimulation may be indicated by the difficulty of extinguishing a C.R., the magnitude of which approximates to its U.R. (Razran, 1956).

In the Skinner box situation 'disinhibition' has proved to be difficult or impossible to demonstrate (Exps. 1 and 2, Skinner, 1938). Here the animal is free to move about, and it is less likely that any extra stimulus will restore the distinctive context of the acquisition trials. Merely alerting the animal may have little effect. It may in fact be actively preening or scratching or re-exploring its surroundings. 'Disinhibition' effects will normally be produced only when the stimulus starts off the early phases of the learned response. In the case of the free operant, giving free reinforcement re-establishes a chain. In the case of discrete responding (Exps. 5(a) and 5(b)) a stimulus that makes the animal move forward in the box may be effective for the same reason. In Exp. 2, the only stimulus that might be judged to have had a disinhibiting effect was the rattling of food, and this as a stimulus did in fact produce movement towards the food dish.

The disinhibition theory does not provide an alternative explanation of the relative difficulty of demonstrating extra stimulus restoration of responses in the operant conditioning situation, nor does it account for the special effects of presenting the reinforcer. Even the straightforward account of 'disinhibition' involves additional hypotheses to explain why extra stimuli affect inhibitory processes more than the excitatory processes they suppress, in Hull's (1952) terms, sI_R more than sH_R . Konorski (1948), acknowledging the extra effectiveness of the U.S. as a 'disinhibitor', attributes this to 'inertia of the food centre', thus endowing a pseudo-neurological entity with pseudo-mechanical properties.

(2) *Speedy reconditioning after extinction*

Since reinforcement can never be withheld without altering the stimulus situation its reintroduction, even after complete extinction, can be expected to disclose some effect of the original training. The magnitude of the effect depends on the locus of extinction and this in turn is determined by the characteristics of the learned response as a chain.

When a response is made up of a number of units as a chain, the early units will be more remote from reinforcement than the later units and so can be expected to show less resistance to extinction. Further, in normal responding the occurrence of later units depends on the occurrence of earlier units. When the probability that an earlier unit will lead to a later unit is reduced below certainty, the later unit will in fact be performed less frequently than the earlier one and as a result will be less subject to extinction effects. Extinction procedures are unlikely then to extinguish every part of a response. As an example from outside the laboratory, if piano-playing ceases to be reinforcing, the break will probably take place at the stage of sitting down at the keyboard leaving the whole skill of playing intact. In general, the core of a skill is rarely exposed to extinction. The rat in a Skinner box may fail to respond when the early phases of his approach to the lever have been extinguished. Reconditioning will be fast because it does not involve setting up the whole response from scratch. An inhibition theory of extinction is not required.

(The stimuli that link a chain of responses need not be kinaesthetic. For a pigeon in a situation of the type used in these experiments, it seems likely that they would be predominantly visual. Even small movements in a Skinner box may be accompanied by large-scale visual changes. In a pilot experiment with four birds, the response of stepping on to a platform 2 in. from the floor was established. The platform was then removed and two birds developed the new response of standing where it had been and stretching their necks, presumably thereby reproducing the visual stimulation that was present at the time of reinforcement when standing on the platform.)

(3) *'Spontaneous recovery'*

It is regularly observed that the performance of an animal at the *beginning* of a second extinction period may be more similar to its performance at the *beginning* of the first extinction period than to its performance at the *end* of the first extinction period. This is called 'spontaneous recovery'. It is not surprising that it should occur since the situation at the end of one extinction period is different from the situation at the beginning of the next. This factor has been stressed by Skinner (1950) and Guthrie (1952). Skinner (1953) goes on to show that pigeons can discriminate their position on different schedules

of reinforcement in terms of elapsed time and number of responses made since last delivery of food. In experiments where trials are massed and responses are effortful, the difference in stimulus conditions between the beginning and the end of a period will be more marked and so more 'spontaneous recovery' will occur. During acquisition periods, reverse effects may occur. For example, a discrimination may be made with greater accuracy at the end of one period than at the beginning of the next. With pigeons these 'warm up' effects may be greatly reduced when the beginning of the experiment is made less distinctive by giving 'free' reinforcement before the stimuli are presented for the first time each day.

Finally, some of the concepts used in current theorizing are sources of confusion since they are remote from the details of behaviour. For example, the term 'habit strength' is used in an absolute way as if responses could be measured for strength like bars of steel. In particular, if the persistence of a response when reinforcement is withdrawn is taken as a measure of habit strength it should be recognized that the 'S' in sH_R during extinction is not the original 'S' of acquisition. The case where only one acquisition or extinction trial is given per session may be an exception, and it is interesting to note that under such conditions the concept of 'spontaneous recovery' has no meaning. Under more normal conditions of successive trials a test of the persistence of a habit is always a test of its generalization to stimulus conditions that are different from those of acquisition. The result can be regarded as a measure of the organism's discrimination. Thus, the finding that responses reinforced intermittently are more persistent than responses reinforced on every occurrence, gives no indication that response strength in any absolute sense is greater. The change in situation when all reinforcement is withheld is less marked in cases where reinforcement was sometimes withheld during the acquisition (Skinner, 1950). The approach that has been outlined may deprive inhibition theory of some of its justification, but it is not strictly incompatible with the theory. In part, at least, inhibition theory may yet prove to be necessary. But it would be unwise to interpret extra stimulus recovery as a complex 'inhibition of inhibition' without first asking whether something simpler will do.

REFERENCES

- BRENNER, J. M. T. (1957). An experimental investigation of the relationship between conditioning and introversion-extraversion in normal subjects. M.A. Thesis, University of Aberdeen.
- GUTHRIE, E. R. (1952). *The Psychology of Learning* (rev. ed.). New York: Harper Bros.
- HULL, C. L. (1952). *A Behavior System*. New Haven: Yale University Press.
- KONORSKI, J. (1948). *Conditioned Reflexes and Neuron Organization*. Cambridge University Press.
- PAVLOV, I. P. (1927). *Conditioned Reflexes* (trans. G. V. Anrep). London: Oxford University Press.
- RAZZAN, R. (1956). Extinction re-examined and re-analysed: a new theory. *Psychol. Rev.* **63**, 39-52.
- REID, R. L. (1952). A test of sensory pre-conditioning in pigeons. *Quart. J. Exp. Psychol.* **4**, 49-56.
- SKINNER, B. F. (1938). *The Behavior of Organisms*. New York: Appleton-Century-Crofts.
- SKINNER, B. F. (1950). Are theories of learning necessary? *Psychol. Rev.* **57**, 193-216.
- SKINNER, B. F. (1953). Some contributions of an experimental analysis of behavior to psychology as a whole. *Amer. Psychol.* **8**, 69-78.

(Manuscript received 22 May 1957)

VISUAL SEARCH FOR SUCCESSIVE DECISIONS

BY N. H. MACKWORTH AND J. F. MACKWORTH

Medical Research Council, Applied Psychology Research Unit, Cambridge

When a situation provides information from many different independent sources, characteristic difficulties arise, even when the decisions required are of the simplest possible kind and do not require estimates of probability. This paper considers the effect of the amount of material in the display which must be searched for the correct answer, and also how this display load interacts with the speed at which decisions are required, in a situation where successive decisions are posed at approximately equal temporal intervals.

It is shown that the percentage of errors is proportional both to the required speed and to the display load, and it is concluded that the time required to search through a display depends on the amount of material in the display, when this unwanted material closely resembles the required answer. In the discussion, the effects of many different sources of information in a moving display are considered in relation to two distinct variables: (1) the display load, and (2) the temporal irregularity of successive decisions.

I. INTRODUCTION

It is possible to study human responses to a complex environment in many different ways. For example: (1) In detailed controlled experiments the environment can be so simplified that as far as possible only one variable is being changed at a time in a limited situation; and with this approach a great deal of accurate information can be obtained about the effects of that single variable. (2) Alternatively, observational studies can be made of people working in their normal environment, in which case the findings are largely descriptive, since a given environment seldom repeats itself exactly. (3) Thirdly, however, simulated tasks provide an intermediate approach in which a fairly complex task is devised for use in laboratory experiments. This simulated task resembles the real situation in certain respects because both the real and the simulated forms of work contain common dimensions of difficulty, which can be altered in a controlled fashion. The interpretation of evidence from simulated tasks suffers from some lack of clarity in relation to the first approach mentioned above, and some lack of reality in relation to the second approach. The simulation experiment appears, however, to be an essential stage in bridging the gap between these two more conventional procedures, so that eventually the precise information obtained by very exact and controlled experiments may be applied with insight to the interpretation of observations from real situations.

A human problem of some importance occurs when action has to be taken on information coming at the same time from several different places in the environment. These sources may be quite independent or they may be related in some way. It may be easy or difficult to decide which source requires attention most urgently. Examples of multi-source displays from real life are found in many dial-watching tasks, a form of work which has been experimentally studied by Conrad (1951, 1954*a*, *b*, 1955*a*, *b*). Other instances, requiring still more difficult decisions, are the complex electronic displays that provide information to air traffic controllers. The positions of aircraft often have to be altered after these electronic maps have been carefully scrutinized. Success in such work may

at times be hard to attain, and, paradoxically enough, many of the difficulties arise from the presence of too much information in the display (Mackworth, 1949).

The following experiments are aimed at an understanding of some of the possible general reasons for these difficulties. It seems important to try to separate the main variables by devising new experimental situations. These laboratory tasks have to give exact and consistently reproducible results in circumstances which nevertheless avoid the dangers of constraining human actions to such an extent that the experimental situation involves only very simple forms of behaviour. The suggested method provides one way of analysing the factors arising when people are searching for a swift succession of decisions and are choosing these from a complex and crowded visual world. This information is quickly changing all the time for minutes on end; and it is often also relatively unambiguous, and does not necessarily entail probability judgements to any extent.

The average speed at which successive actions are demanded is not the only way in which work of this kind can become difficult. Two further variables have been isolated from such tasks—temporal irregularity and display load. Both these are characteristic of situations in which there are many sources of information. When a number of separate sources of information are generating stimuli independently at different rates, there are periods with many stimuli close together in time and these busy spells may be followed by long blank pauses (Cox & Smith, 1953). This temporal irregularity with its resulting tendency for the signals to overlap with each other along the time scale has been considered in a previous paper (Mackworth & Mackworth, 1956). The present work concentrates on two other variables—the average demanded speed of decision and the display load which is the amount of material simultaneously visible that must be searched for the correct decision.

II. METHOD

(a) *Nature of the order search task*

The experimental situation was designed to provide a mass of detail from which the subject had to pick out the one objective that was appropriate at any given time. As soon as this present objective had been identified by visual search, the action required on it was very simple and the next objective had to be sought (Mackworth, 1948).

The basic task was to bring a number of counters to the right places at the right time. Each man was tested by himself and sat in front of a visual display on which were shown: (1) a three-figure clock; (2) a large card marked off in half-inch squares; and (3) a set of counters placed on this grid. Each of these counters was labelled with its own reference letter, as in Fig. 1.

Each counter had to be moved down its own column on the grid. In this column there were four objectives, each marked with the time by which that particular square had to be reached. This expected time of arrival was given as a three-figure number in the same way as the readings on the clock.

At regular intervals the clock moved on one digit and ticked loudly whenever it did so. The subject had to give one order for each tick; he called out the index letter of the counter he wished to be moved so that the experimenter could move it on manually one square down the column. The subject was therefore searching for the forthcoming objectives in their correct sequence at a speed of work determined by the clock. He had one decision to take for each objective.

Fig. 1 shows an example of one of the displays. In this particular presentation there are twenty counters to be moved. As the clock ticks on from '000' the subject should name counter *I* twice for the first two ticks, as a result of his first decision to select the objective '002'. Then the counter *T* should be named three times after the second decision to pick objective '005'. Then he should name *M* repeatedly to achieve objective '015', and *Q* to reach '017'. Periodically some counters had to arrive early to ensure that subsequent counters arrived on time. Such early arrivals were not penalized in the present study. Thus the subject could not verify that he had the correct objective by counting the moves still required to

reach it; he had to consider every objective to make sure there was not another one even slightly earlier than the one found so far—unlike the more usual number search tasks. However, if the task was being performed correctly, the search was limited to the next objective in each column immediately beyond the counter.

The subject could obtain more time for his search by looking ahead. This involved searching for the next objective while calling out the moves already decided on to achieve the present objective.

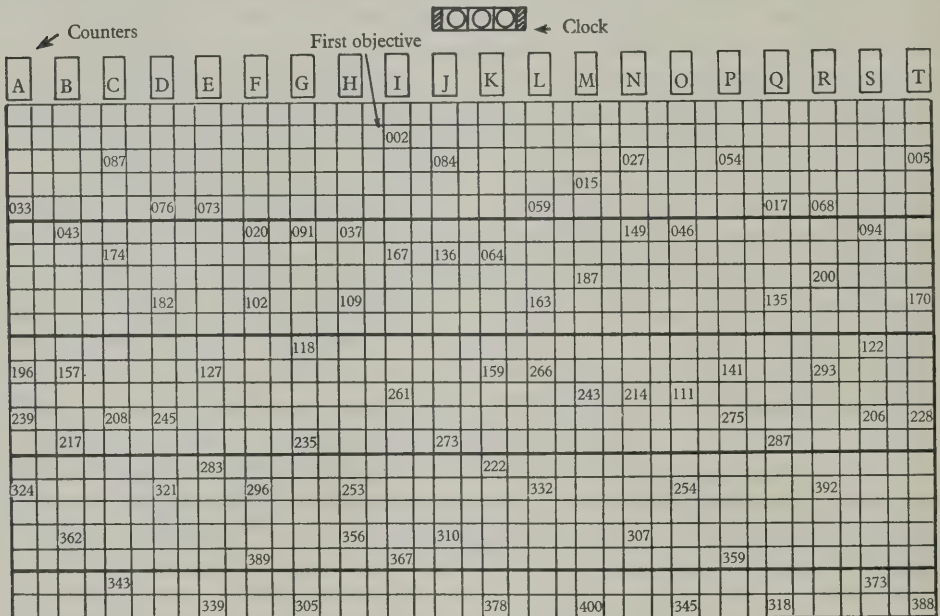


Fig. 1. Order search display, example of heavy load—20 columns.

(b) Scoring method

The method of scoring was to record, at each of the expected times of arrival, the difference between the expected and actual positions for that particular counter at that instant. This was expressed as the number of squares that the counter lay short of, or beyond, the objective. The experimenter dictated these numbers on to a tape recorder. The total error score was expressed in terms of errors per man per 100 moves, which represented 20 decisions. This was calculated by dividing the total errors per run by the number of moves per run (i.e. 100 moves for 5 columns to 400 moves for 20 columns).

By taking observations at each expected time of arrival, a semi-continuous record of achievement was kept during each run. It was specifically intended, for the sake of realism, that early errors should be allowed to prejudice later performance, i.e. that counters not in their correct position should further confuse subsequent action.

(c) Experimental variables

The variables studied were the *average demanded speed of decision* in terms of the average number of decisions per minute, and the *display load*, which for this experiment was defined as the number of columns through which visual search had to be made to find the present objective. Often, in practice, additional sources of information are forced into the environment without allowing the subject any extra time. It was desirable, however, to consider what happened when each of these two factors of required speed and display load was altered separately, as well as their effects when both were increased together.

For the first experiment, twenty conditions were considered since there were five demanded speeds of decision and four levels of display load. The five *speeds* were achieved by making the clock tick at different rates. The fastest speed entailed 12 decisions per minute, and the slowest only 2.4 decisions per minute.

In terms of moves of the counters, this meant a move every second or a move every 5 sec. (The search for a further objective had to be made within the time available for one move, unless the subject looked ahead and searched while he was calling out the present answer.)

The four *display loads* were obtained by increasing the number of columns presented. There were five, ten, fifteen or twenty columns. Each column always contained four objectives. (The columns were spaced out regularly across the card, so that the outermost columns always lay near the edge of the card, which measured 25 in. wide and 13 in. high, and subtended a visual angle of $\pm 35^\circ$ horizontally and $\pm 13^\circ$ vertically, since the eyes were about 20 in. from the top of the card.)

(d) *Experimental arrangements*

For the first experiment, the statistical design used five groups of four men. Each group did all the twenty conditions. The members of each group were allocated at random to the four levels of load, but each man did each speed, in random order. Thus each man did five different conditions once.

The length of run for each display varied from 100 to 500 sec. for the five columns, and from 400 to 2000 sec. for the twenty-column display.

For the second experiment twelve conditions were tested, six levels of load, which ranged from five to fifty columns, at each of two speeds, viz. three and six decisions per minute. Each man did all the conditions in a Latin square arrangement. Six of these were in the morning and six in the afternoon.

Forty-four naval ratings, aged eighteen to twenty-nine, took part in this pair of experiments, twenty men for the first and twenty-four for the second. All were given an individual demonstration, followed by a practice spell of an hour and a half.

III. RESULTS

(a) *Results from Experiment 1*

Table 1 gives the average error score per 100 moves at each of the experimental conditions.

It can be seen from Table 1 that both the required speed and the display load have a considerable effect on the error scores. Fig. 2 shows the relation of display load to errors when the speeds were grouped into fast and slow, and Fig. 3 shows the relation of speed to errors when the display load values are grouped into high and low.

Table 1. *Average error scores per 100 moves in Experiment 1*

Speed (decisions required per minute)	Display load (number of columns)				Means
	5	10	15	20	
2.4	1	4	3	5	3
3.0	0	3	5	13	5
4.0	3	7	16	18	11
6.0	6	14	28	20	17
12.0	25	29	54	72	45
Means	7	12	21	25	—

Owing to the dependence of the variance upon the means in these results, it was necessary to apply a transformation to the readings. The simplest transformation which would remove this dependence was the use of the square roots of the errors. Table 2 shows the average square roots of the errors.

The analysis of variance on the group square root totals at the five speeds is given in Table 3.

This demonstrates the highly significant effect of the required *speed* on performance, and also that the differences between group totals at the same speed are not significant. This suggests that subject differences are not important when we take the average of four readings. The averages given in Table 2 are the averages of five results, so that an

analysis of variance is probably justified and this analysis of variance of the transformed data is therefore given in Table 4.

These results demonstrate the highly significant effect of *display load* upon performance.

It is important to check whether these differences obtained between the various display loads are due to the extra length of the task with the heavier loads. One way of studying

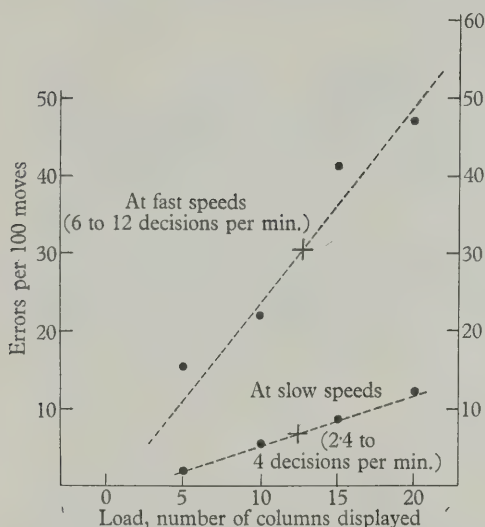


Fig. 2

Fig. 2. Effects of increasing the display load.

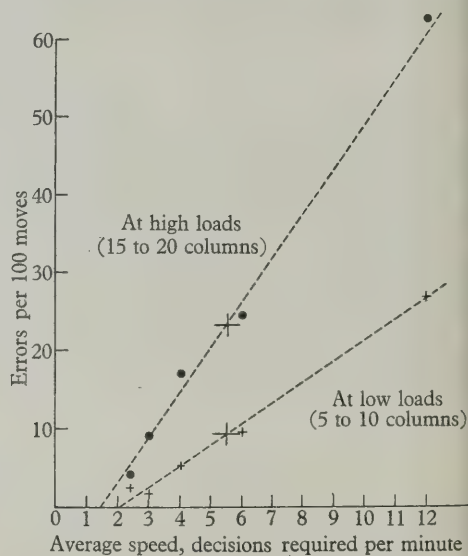


Fig. 3

Fig. 3. Effects of increasing the required speed of decision.

Table 2. *Averages of square roots of errors in Experiment 1*

Speed (decisions required per minute)	Display load (number of columns)				Means
	5	10	15	20	
2.4	0.5	1.5	1.8	1.7	1.4
3.0	0.0	1.7	2.1	3.4	1.8
4.0	1.2	2.6	3.7	4.1	2.9
6.0	1.8	3.4	5.0	4.3	3.6
12.0	4.3	5.2	7.3	8.3	6.3
Means	1.6	2.9	4.0	4.4	3.2

Table 3. *Analysis of variance of group square root totals in Experiment 1*

Source	D.F.	S.S.	M.S.	VR	Significance
Speeds	4	90,323	22,581	41.8	< 0.01
Groups	4	3,895	974	1.8	N.S.
Error	16	8,644	540	—	—
Total	24	102,862	—	—	—

Table 4. *Analysis of variance of Table 2 from Experiment 1*

Source	D.F.	S.S.	M.S.	VR	Significance
Speeds	4	59.7	14.9	41.4	< 0.01
Loads	3	23.8	7.9	22.9	< 0.01
Error	12	4.3	0.36	—	—
Total	19	87.8	—	—	—

this possibility is to consider only the errors in the first 100 moves for each of the various display loads. (This method of analysis *underestimates* any difference between high and low loads since, with the twenty-column load, the first 100 moves refer only to the first objective in each column, whereas with the five-column load the errors are cumulative through four objectives in each column.) Nevertheless, a regular increase in the average error score per hundred moves is again found, as can be seen in Table 5, which is averaged over the various speeds.

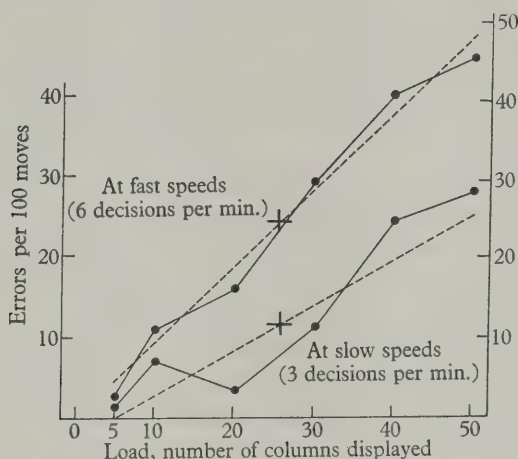


Fig. 4. Effects of greatly increasing the load.

Table 5. *Average error score during first 100 moves in Experiment 1*

Display load (number of columns)			
5	10	15	20
7	9	11	13

Table 6. *Average error scores per 100 moves in Experiment 2*

Speed (decisions required per minute)	Display load (number of columns)						Means
	5	10	20	30	40	50	
3	1	7	3	11	25	28	12.5
6	3	11	16	29	41	45	24.2
Means	2.0	9.0	9.5	20.0	33	36.5	—

(b) *Results from Experiment 2*

Fig. 4 and Table 6 give the results of the second experiment in which the display load was greatly increased from five to fifty columns. The two lines on Fig. 4 show the errors per 100 moves for the two speeds plotted against the display load.

IV. DISCUSSION

(a) *The order search task*

In this laboratory situation the time-table numbers indicate the objectives for the search, and specify these in space as well as in time. The subject has to select these objectives in a sequence, and the clock indicates which of the objectives is the present objective at

any given moment. To bring each counter to the right place at the right time, the subject has to note the present clock reading and to calculate quickly the differences between this and the times given by the various possible objectives. As soon as he has found the objective that is the first one after the present clock reading, he calls for action on the counter aimed at that objective.

In other words, when he is making a decision, the subject starts with the differences in these time readings; and each of these time differences can be termed a temporal mismatch. MacKay (1956) has considered how a controlling system works from moment to moment in selecting what an effector system should do next, by choosing amongst the various possible lines of action. The effective stimulus is a mismatch. This is a difference between the desired and actual state of affairs, a mismatch which man is usually trying to minimize.

As in many real life situations, the main difficulty in selecting the correct action lies in the fact that all the possible objectives resemble each other. There is no difficulty in reading these three-figure numbers. The main problem is that the present objective can be identified only after the subject has rapidly considered a whole group of potential immediate objectives in relation to the present clock reading.

The nature of the task may change as mistakes accumulate, and the run proceeds. If the counters are being moved correctly, the task could be performed without looking at the clock, the subject merely having to find the next number in a sequence, but as soon as some objectives have been missed, then the correct move can only be found by considering and comparing the various temporal mismatches.

(b) Relationship between speed and load

(1) It has been statistically proved that both load and speed are important factors in the order search task. The display load has been defined here as the number of columns in the display through which visual search has to be made to find the present objective—and the speed is regarded as the average number of decisions required per minute. The result proved that error scores increase when the complexity of the display is increased in this way, even when the number of decisions demanded per minute is held constant.

(2) The results also suggested certain relationships which cannot be statistically proved on the present data. These indications are discussed below in paragraphs (3) to (7) inclusive, and need further experimental work.

(3) Performance in terms of errors per 100 moves would appear to be linearly related to the display load, provided the demanded speed of work is kept constant. Figs. 2 and 4 indicate that this linear relationship seems to hold good up to the display load of at least fifty columns, which was the highest load investigated.

(4) The results in Fig. 3 suggest that performance in terms of errors per 100 moves is also linearly related to the average demanded speed of work measured in decisions per minute, given that the display load is kept constant.

(5) Fig. 2 suggests that the effect of display load is greater at the higher speeds, and Fig. 3 that the effect of speed is greater at the higher loads. It would therefore seem that the effects of demanded speed and display load are not simply additive, i.e. the effect of increasing one factor intensifies the effect of increasing the other, to give diverging rather than parallel lines in Figs. 2 and 3.

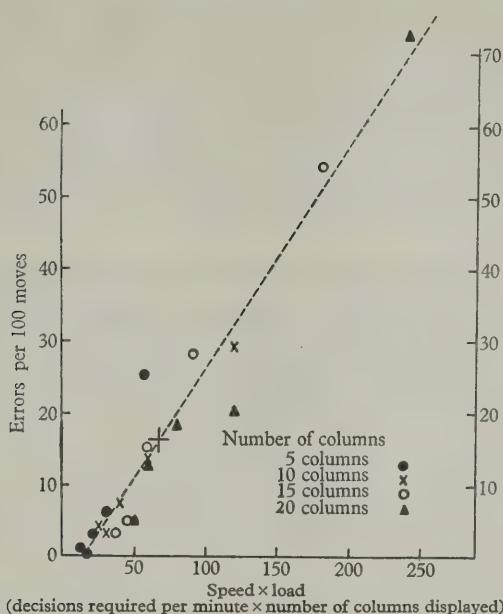


Fig. 5. Relation between errors and product of speed and load

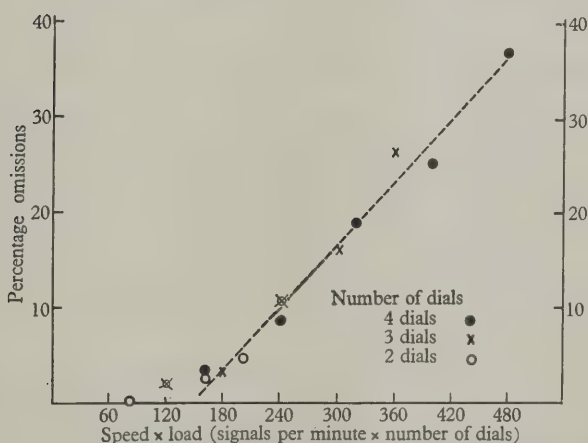


Fig. 6. Relation of percentage omissions to product of speed and load. Data from Conrad (1951).

(6) Inspection of the results shown in Table 1 suggests that in the circumstances of the experiment it is possible to obtain the same error scores when the speed is doubled and the load is halved. When the product of speed and load (as measured in the way stated) is constant, the error score is also fairly constant. (Approximately the same errors are obtained, for example, at a speed of 3 decisions per minute with a load of 20 columns as are found with a speed of 6 decisions per minute and a load of 10 columns. Similarly, an almost identical score is found with a speed of 4 decisions per minute and a load of 15 columns.) The relationship remains true even when, as in Table 2, the individual results are transformed into square roots to give a more normal distribution.

(7) These results suggest that the errors per 100 moves might be linearly related to the product of speed and load, and this relationship has been plotted in Fig. 5.

(8) Similar relationships can be found in the literature, e.g. Conrad (1951) in his work on multiple dials obtained results which have been re-calculated and shown in Fig. 6; this plots the product of demanded speed and display load (in terms of decisions per minute and number of dials) against percentage omissions. A straight line can be fitted to the data when the percentage omissions are greater than about 2%.

(c) Search time and load in static displays

It is thought that the time to find an objective depends directly on the number of objectives that must be inspected, because the subject takes a certain time to examine each objective and reject it. Eriksen (1955) has shown that when searching for ten objects, namely triangles, amongst other different shapes, such as squares and diamonds, the time to find the triangles was directly proportional to the number of other irrelevant shapes in the display, which contained a total of 20, 40, 60 or 80 shapes.

Similarly, Green & Anderson (1956) have noted that the time required to find a given numeral in a display is directly proportional to the number of other numerals in the display. It is therefore reasonable to suppose that a similar effect is occurring in the order search task, and that errors will occur when the pacing is too fast for the subject to find his present objectives. The main point is that failure is just as likely to arise from greater display complexity with more details to discard, as from demanding more decisions per minute.

(d) Relation between the present objective and the rest of the display

Some situations need hardly any search at all, as when a bright light is presented against a neutral background—e.g. a searchlight on a dark night. At the other extreme, no amount of search will reveal the wanted signal when this is identical with other unwanted parts of the display, e.g. when a weak target signal on a radar tube is quite indistinguishable from unwanted random flashes. Between these two extremes there are many instances in which visual search is rewarding but difficult. Such difficulties arise when the unwanted items in the display are more numerous than the present objective, and also resemble it in certain respects.

The unwanted items are not necessarily totally irrelevant in the sense that they are never going to be required. For example, in a serial task such as the order search situation, all the information shown on the display is relevant at some stage during the problem. At any given moment in the task, however, most of the information shown has to be discarded by the subject, not because it is entirely random information, but rather because it concerns past objectives or objectives which are far into the future. This has been shown to be harmful to performance. It is therefore important to remove from displays all the information that is not needed for present action. The less the predictive behaviour entailed by the particular series of decisions, the greater is the need for a display showing only the present objectives (Mackworth, 1956). This implies: (i) the removal of past objectives, (ii) reducing the number of future objectives, as well as (iii) the traditional elimination of 'noise' or random information. Electronic computers can help by storing information as it comes in, and by displaying only present objectives, these being shown just before the operator has to consider them.

Even if, however, it is not possible to have one of these 'now' displays, much can be done by some special labelling for the present objectives. Surprisingly little is known about the factors that lead to confusion between visual objects; it is however clear that the difficulty of search is related to the similarity between the present objective and the rest of the display material. For example, Green & Anderson (1956) have demonstrated that differential colour labelling of numerals effectively reduces visual search to those numerals in the given colour.

(e) Spatial search and temporal irregularity in multi-source displays

It is believed that there are two main sources of difficulty characteristic of a multi-source display (apart from the average speed required); these are (1) temporal irregularity, and (2) display load. Temporal irregularity has been reduced to a minimum in this order search task, while the effect of varying the display load at different required speeds has been studied in some detail.

On the other hand, there are undoubtedly a great many real-life situations in which signals are displayed independently from many sources so that both factors are present. The laboratory situation mentioned earlier, devised by Conrad (1951), presents a task involving both temporal irregularity and varying display load (Conrad, 1955*b*). With temporal irregularity, a marked increase in the demanded speed of work tends to reduce the disadvantage of increasing the number of channels, which is at a maximum at an intermediate speed. This can be seen in Fig. 4 of the paper mentioned above on signal overlap (Mackworth & Mackworth, 1956). Raising the demanded speed of work has exactly the opposite effect in the present laboratory task which involves searching the display; faster pacing increases the disadvantage of more channels. Thus in a complex task, the exact effects of demanded speed on multi-source displays may depend on the relative importance of temporal irregularity and display load.

This discussion has considered the relation between speed and load in terms of an error criterion averaged over a given number of moves or decisions. Quite a different picture is obtained when the score is measured in terms of the average number of responses *per unit of time*. Conrad (1954*a*) has pointed out that increasing the display load reduces the total output of correct responses per minute—whereas increasing the speed by demanding more decisions per minute may have the opposite effect of increasing the output of correct responses per minute.

Inevitably, therefore, when the measure of performance is the output of correct responses *per minute*, it is impossible to compensate for an increase in display load by decreasing the required speed, since both these changes tend to reduce the output per minute.

The importance of avoiding unduly heavy display loads is greatest in situations where it is essential that every decision must be made accurately at the right moment. Such a situation is more likely to be found in, for example, air traffic control than in most existing industrial situations where the yardstick is productivity per unit of time.

V. CONCLUSIONS

This experiment is a contribution to an analysis of the effects of increasing the number of sources from which the subject is expecting visual information about which he must decide. Consideration of the difficulties produced by increasing the number of sources has identified two main causes of failure, namely temporal irregularity and display load—apart from any other effects due to an increase in the average number of decisions demanded in unit time.

In a situation in which *temporal irregularity* is the limiting factor for performance, the main obstacles to success are believed to be the overlapping of signals for decisions during very active periods, followed by enforced pauses in which no action is required. Search problems are at a minimum since all the signals are easily seen and must be responded to as soon as possible after they appear. There is no need here for the concept of display load, because performance decrement can be accounted for by the moment-to-moment variations in the demanded speed of work. These variations are superimposed on the general average demanded speed.

On the other hand, *display load* may be the limiting factor when the display is a complex one, and there is difficulty in deciding which is the present objective. This is intensified when there is an increase in the difficulty of distinguishing between the wanted and unwanted information. The concept of display load is essential in describing visual search; in the task considered in this paper, *display load* has been defined as the number of columns through which visual search must be made to find the present objective.

It has been shown that errors per 100 moves in the order search task depend directly upon the display load as well as upon the speed. Moreover, it seems that the effects of display load tend to increase as the speed increases. This is in opposition to the findings with temporal irregularity, where increasing the average speed beyond a certain point tends to diminish the effects of temporal irregularity introduced by extra sources.

When accuracy is all-important to the visual search task and when percentage error is the method of scoring, it is possible to demonstrate some convertibility in the speed and load factors, in the sense that halving the speed may compensate for doubling the load, i.e. halving the number of decisions required per minute may compensate for doubling the number of columns on the display. It should be pointed out that since the speed in terms of the number of decisions demanded per minute is a product of the number of sources and the speed at which each source is requiring decisions, then whenever the number of sources is increased, the rate at which decisions come in *from each source* must be reduced very considerably to hold constant the product of average speed and load.

These researches owe a great deal to the encouragement and comments received from Sir Frederic Bartlett, F.R.S., as well as to the advice obtained from our colleagues during many discussions. The statistical design of the experiments was made by Mr E. G. Chambers and the analysis was by Mr Mervyn Stone.

VII. REFERENCES

- CONRAD, R. (1951). Speed and load stress in a sensori-motor skill. *Brit. J. Indust. Med.* **8**, 1-7.
- CONRAD, R. (1954*a*). Speed stress. Chapter 13, in *Human Factors in Equipment Design*. London: H. K. Lewis and Co. Ltd.
- CONRAD, R. (1954*b*). Missed signals in a sensori-motor skill. *J. Exp. Psychol.* **48**, 1-9.
- CONRAD, R. (1955*a*). Timing. *Occup. Psychol.* **29**, 173-81.
- CONRAD, R. (1955*b*). Some effects on performance of changes in perceptual load. *J. Exp. Psychol.* **49**, 313-22.
- COX, D. R. & SMITH, W. L. (1953). The superposition of several strictly periodic sequences of events. *Biometrika*, **40**, 1-11.
- ERIKSEN, C. W. (1955). Partitioning and saturation of visual displays and efficiency of visual search. *J. Appl. Psychol.* **39**, 73-7.
- GREEN, B. F. & ANDERSON, L. K. (1956). Color coding in a visual search task. *J. Exp. Psychol.* **51**, 19-24.
- MACKAY, D. M. (1956). Towards an information-flow model of human behaviour. *Brit. J. Psychol.* **47**, 30-43.
- MACKWORTH, N. H. (1948). Researches on the measurement of human performance. Research Fellowship Thesis, St John's College, Cambridge.
- MACKWORTH, N. H. (1949). Human problems of work design. *Nature, Lond.*, **164**, 982-3.
- MACKWORTH, N. H. (1956). Work design and training for future industrial skills. (Sir Alfred Herbert 1955 Lecture.) *J. Instn Prod. Engrs*, **35**, 214-40.
- MACKWORTH, J. F. & MACKWORTH, N. H. (1956). The overlapping of signals for decisions. *Amer. J. Psychol.* **69**, 26-47.

(Manuscript received 13 May 1957)

THE ROLE OF ATTITUDES IN COMPREHENSION

By S. FRIEDMANN

Child Guidance Clinic, City of Oxford

By means of an attitude test and expressed opinions, subjects were divided into pro-, anti- and neutral-groups for each of twenty-four political statements. No significant differences in comprehension were found between the three groups, except in two cases where the groups who scored higher for comprehension also had superior intelligence and knowledge scores. A significant result was found for only one of the twenty-four statements, namely, a negative correlation between liking and comprehension, and a comparable superiority in comprehension of the opposing group: this statement was found to be ambiguous in its expression of attitude.

The findings of a control experiment, using comments instead of a comprehension test, supported the main conclusion that for unambiguous statements there was no relationship between the efficiency of comprehension and direction of attitude.

I. INTRODUCTION

In spite of the growing interest in the relation between affective and cognitive processes, there have been very few experimental studies on the influence of attitudes on comprehension. It is often held that in order to understand an idea one must be in sympathy with it (Baumgarten-Tramer, 1941; Hippus, 1936; Hodges, 1944; Marum, 1936), or at any rate, that favouring an idea leads to better comprehension than does antagonism to it. On the other hand, this view has been strongly attacked on the grounds that understanding depends on knowledge and skill, and that favourable attitudes are quite irrelevant (Ryle, 1949). It might equally well be the case that neutral attitudes, or a critical approach, lead to better appreciation. The issue can only be decided experimentally.

Experimental studies of the relation between affective factors and cognitive processes have been mainly in the fields of learning, recognition, and remembering. There are several investigations of social attitudes—in the sense in which Sherif & Cantril (1945, 1946), used the term—which purport to show that favourable attitudes lead to better learning and remembering than unfavourable attitudes. Edwards (1941) tested the recognition of items in a New Deal speech of subjects with pro- and anti-New Deal attitudes. The favouring groups scored consistently better than antagonistic groups, although the differences were not statistically significant. Seeleman (1940) used two groups of subjects with pro- and anti-Negro attitudes in the recognition of Negro and White pictures. The anti-Negro group recognized fewer Negro pictures, and fewer individual differences between Negroes, and remembered more unfavourable than favourable phrases connected with Negro pictures than did the pro-Negro group. Levine & Murphy (1943) found Communists superior in learning and remembering a Communist passage to anti-Communists, while the latter scored better on an anti-Communist passage. Most of these investigators interpreted their findings in terms of Sherif & Cantril's concept of reference frames. It was contended that material which is congruent with the individual's frame of reference is more easily assimilated than material which is not.

Since learning, recognizing, and remembering are involved in understanding, these experiments might be deemed to build up a *prima facie* case for the contention that favourable attitudes lead to better comprehension. There is little direct evidence,

however. In a study of the intelligibility of spoken messages, Mason & Garrison (1951) obtained significantly higher scores for liked than for disliked sentences. Inspection of the material suggested to them that liking was more a matter of greater familiarity than of favouring. Cooper & Jahoda (1947) held that 'it is difficult in general for a communication to reach people who are not already in favour of the views it presents'. Subjects with anti-minority views misunderstood a series of cartoons ridiculing anti-minority feelings, and misconstrued the point so as to fit it in with their own convictions. This may well be a special case, however, since the subjects had to see themselves as ridiculous if they understood the cartoons. Unfavourable attitudes can be aroused quite strongly without necessarily threatening the subject's self-esteem. The results, therefore, do not necessarily support Cooper and Jahoda's more general hypothesis. Doob (1947) criticized experiments on social attitudes in learning, recognizing and remembering for not controlling for previous knowledge of the subject-matter. The findings might reflect no more than the better knowledge of the favouring groups. Alternatively, the results could be explained by assuming greater interest—hence better incentives—in the favouring groups. Thus, there is but slight evidence on which to base a theory of the relation between attitudes and comprehension. Most investigators demonstrate that the reproductions and interpretations of material reveal the subjects' attitudes, but it is by no means clear what functions these attitudes have, or how they are related to the efficiency of comprehension. The present investigation sought to determine whether there is any relation between the efficiency with which subjects understand statements, and their attitudes to the opinions expressed in them.

II. EXPERIMENTAL MATERIAL

Short political statements from publications of the major British parties were considered the most suitable material, since they are designed to arouse the kind of attitudes here to be investigated. In order to see if any possible effect of attitude on comprehension is connected with the level of difficulty of the material, the sentences were to be at three levels—easy, average, and very difficult. Over a thousand typical statements were collected from Conservative, Liberal, Labour and Communist party pamphlets. The experimenter and two other observers selected eighty of these as falling most consistently into five grades of difficulty. Sixty-two psychology students then ranked the eighty statements for difficulty. A further sixty-five subjects with a wide range of political opinions grouped the statements according to the four political parties, to ensure that, although taken out of their context, the sentences still expressed the views of the parties from which they issued. The two statements which ranked consistently in the first, third, and fifth grades of difficulty were selected from each of the four attitude groups. These twenty-four statements, printed on separate cards, were used in the investigation (see Table 1).

Table 1. *Statements for Comprehension Test (Material used in 1950)*

Easy statements

No. 36F. (Conservative.) Thousands of men and women would work harder today if they could look forward to buying a house with their savings.

No. 19L. (Conservative.) Working men are not frightfully anxious about having their teeth yanked out for nothing, or their children kept at school until they are of marriageable age.

No. 17D. (Liberal.) The Election Landslide of 1945 came as a surprise to many voters; both to those who voted for the Labour Party and those who voted against it.

No. 71N. (Ambiguous Liberal.) The basic fact about the Liberal Party is that it is opposed to both the present major parties.

No. 104B. (Labour.) In April 1928 the Tories reduced the State grant towards milk for expectant mothers.

No. 104H. (Labour.) On the third anniversary of Labour's General Election triumph, 5 July 1948, the full social security scheme was in force, the Family Allowances, the National Insurance, Old Age Pensions (increased to 26s.), and National Assistance.

Table 1 (*cont.*)

No. 39C. (Communist.) During the war, in every country that was fighting fascism the Communist Parties played a leading part.

No. 56H. (Communist.) The Labour Government carry out a purge against Communism in the Civil Service, but allow Mosley to speak in schools controlled by Labour Councils.

Statements of average difficulty

No. 52C. (Conservative.) Private property is a natural right of the family and generally of the subordinate group.

No. 68A. (Conservative.) As the Conservative sees it, the difficulty of private enterprise lies not in Socialist doctrine but in the size of unit created by modern technical development.

No. 71L. (Liberal.) I do not maintain that an effective union of Europe depends on every government being liberal, but they must not be illiberal.

No. 61D. (Liberal.) According to the orthodox Liberal view, the cause of wars was not Capitalism, but authoritarianism and in particular the authoritarianism of aristocracies and princes.

No. 73G. (Labour.) Under Socialism we shall be able to do with many fewer bureaucrats because the main industries will be organized directly as public services for the common benefit.

No. 63C. (Labour.) There are big industries not yet ripe for public ownership which must, nevertheless be required by constructive supervision to further the nation's needs.

No. 100A. (Communist.) If the electoral programme of the Labour Party in Britain had been carried out in the interests of the working class it would have resulted in real inroads being made into the power of the capitalist class.

No. 40P. (Communist.) The bourgeoisie has subjected the country to the rule of the towns.

Difficult statements

No. 54F. (Conservative.) Conservatives think Liberals failed because they were afraid to anchor their theory of government upon the moral precepts of the natural law and sought instead something more objective which would be more universally recognized.

No. 29A. (Conservative.) The truth of the matter is that European recovery depends not only on preferential arrangements but on a revival of the Sterling Area and the creation of a dollar pool.

No. B1. (Liberal.) Under a well-constituted, or even under a well-administered though ill-constituted government, men's moral sensibility is commonly stronger, and their moral biases more conformable to the dictates of utility.

No. 67S. (Liberal ambiguous.) Inspired with the sacred writings of Adam Smith among economists and of Jeremy Bentham among political theorists, the Liberals of the nineteenth century proclaimed a jihad against all forms of government interference in the realm of commerce.

No. 6F. (Labour.) Only the acceptance of the major changes carried out through the recent Parliament would enable Conservatives to put over effective propaganda to many of the groups which are drawn from those sections of the electorate which in the past supported the Conservative Party as a 'safe' party.

No. 65V. (Labour.) Exactly in proportion as Conservatives believe in federation they distrust the passionate young nationalisms they have largely helped to create.

No. 38B. (Communist.) For many a decade past the history of industry and commerce is but the history of the revolt of modern productive forces against modern conditions of production, against the property relations that are the condition for the existence of the bourgeoisie and its rule.

No. 33E. (Communist.) The proletarians cannot become masters of the productive forces of society except by abolishing their own previous mode of appropriation.

III. COMPREHENSION TEST

'An individual comprehends, or understands a situation', was considered to mean that the individual is able to vary his responses to the situation and can adapt them to variations in the situation in accordance with his purposes. While this may not cover all the senses in which the term 'understands' is used, it would appear to cover all those with which the present investigation is concerned. The behaviour referred to above is exhibited in general by demonstrating verbally or otherwise how a situation hangs together, how it works, what follows from it, what conditions precede it, and how similar new situations will work. The specific tasks involved in understanding statements have been given in the studies of Richards (1930) and Davis (1942, 1944). In order to make these tasks explicit, a comprehension test was constructed in the following form. (i) Explain in detail, wherever possible with concrete examples, in as SIMPLE terms as possible, what this statement means. Also explain any vague or ambiguous words the

statement may contain. (ii) Give reasons why you believe the statement to be true or untrue. (If you have insufficient information to judge whether the statement is true or false, state what kind of facts would make it true or false. In statements that state what somebody believes, give reasons BOTH for thinking whether they believe it, and whether the belief is true or false.) (iii) Is there anything the writer of the statement left unsaid, but which is implied in the statement? If so, what is it? (iv) What effect did the writer of the statement wish to produce on his reader? These questions were duplicated on foolscap sheets, leaving half a page between questions so that answers could be given in essay form (see Vernon, 1950). An observer who answered these questions satisfactorily for a given statement was held to understand it. The satisfactoriness of the responses was judged by three independent examiners who subsequently compared their criteria and pooled the results for the final scores. At the end of the form, space was left for introspective remarks with instructions to the subjects to comment fully on their reactions to the statement, and to give an account of what passed through their minds during the period in which they were trying to understand the statement.

IV. ATTITUDE TEST

'*A* has an attitude to *Y*', was held to mean that *A* tends to act and feel with regard to *Y* in an acquired but characteristic manner. Attitudes were distinguished from interests. '*A* is interested in *Y*', was held to mean that *A* tends to be motivated to attend to *Y*, deal with it, or respond to it. It was considered that attitudes describe the direction, consistency and characteristic manner of the organism's responses. This applies particularly to social attitudes, i.e. attitudes towards values and ideas held in a society. According to Bartlett (1932), such attitudes may be aroused immediately on first general impression, before the material is completely understood, and influence perception. To ascertain whether there is such an influence on understanding statements, subjects were asked the following question immediately after reading a statement. 'Did you like or dislike the attitude expressed or implied in the statement? Indicate your liking or dislike by putting a cross against one of the following: (i) Liked very much; (ii) liked; (iii) moderately liked; (iv) neutral; (v) moderately disliked; (vi) disliked; (vii) disliked very much.' (The ratings were checked to ensure that liking or dislike of attitude, and not of ease or difficulty, had been recorded.)

The observer's general political orientation was ascertained by an attitude scale, and by opinions expressed during the testing and in the scripts. The attitude scale was not envisaged as a linear radicalism-conservatism continuum, but consisted of twelve easy statements expressing Conservative, Liberal, Labour and Communist opinions, respectively. The subjects ranked these in order of preference.

V. POLITICAL KNOWLEDGE TEST

A political knowledge test was constructed, consisting of factual questions about British politics. Fifty-five questions on the names of ministers, parties in power, political literature, and figures of production and consumption were arranged in the form of a four-choice test, and given to eighty Arts, Science and Law students. An item analysis was performed, and the items were scaled for difficulty. Thirty-two questions were selected from those showing a higher correlation with the test and presenting an ascending order of difficulty. The items were so arranged that not too many difficult questions followed each other for the final form of the test.

In addition the N.I.I.P. group test 33 (verbal intelligence) was used.

Taking twice the standard error of the correlation (0.29) as the criterion of significance, the correlation of 0.48 between the N.I.I.P. test and the political knowledge test was considered satisfactory. Correlations between comprehension and the N.I.I.P. test were rather lower than had been hoped (0.30 to 0.50). In five cases, they were somewhat below the criterion. The statement which had been the first to be marked for comprehension showed low correlations both with intelligence and knowledge. Apart from this, the correlations between comprehension and political knowledge were well above the criterion of significance (0.34 to 0.63).

VI. PROCEDURE

Forty-nine subjects took part in the experiment. With the exception of six subjects, they were Arts and Science graduates and undergraduates. Each subject came for five sessions of approximately 1½ hr. Six statements were dealt with in each of the first four sessions, given in the order: easy, average, difficult, average, difficult, easy. In every group of six, there was at least one statement from each of the four attitude groups. After reading the sentence, the subject recorded his like or dislike of the attitude

stated or implied. Then followed the four comprehension questions, and finally a request for introspective remarks.

No time limits for reading or answering were imposed on the first nineteen subjects. It was found more convenient to set time limits for subsequent subjects. These were arrived at by slightly extending the average times of the first nineteen subjects, so that every one would have ample time to finish. Since there were no significant differences in comprehension between the two groups, they were treated together in the analysis.

In the final session, the intelligence, knowledge, and attitude tests were given, in that order. No time limits were imposed for the last two tests. The mean score on the N.I.I.P. test was 156.3 with a standard deviation of 19.3. The mean score on the political knowledge tests was 16.3 with a standard deviation of 5.2.

Most of the subjects were tested individually, some in twos, and there was one group of five subjects.

In order not to prejudice the results, the subjects were told initially that the experiments were made to validate tests. At the end of the sessions, their actual purpose was explained.

VII. SUPPLEMENTARY EXPERIMENT

In case results were affected by the comprehension test which directed attention to various aspects of the statements, a control group of twenty subjects was asked to comment on nine of the statements instead of answering comprehension questions. Eight were medical and social science students, and twelve students of a University Extension class in Social Psychology. The mean score on the N.I.I.P. test was 147.7, and on the knowledge test 17.4. They thus came within the same intelligence and political knowledge category as the main group.

VIII. RESULTS

Four hypotheses may be held about the relation between attitudes and understanding: Comprehension is better when (a) subjects' attitudes are in agreement with the views expressed in the statements; or (b) subjects are opposed to these views; or (c) subjects are neutral in their attitudes to these views; or (d) the efficiency of comprehension does not depend on the agreement, disagreement or neutrality of the subjects' attitudes to those expressed in statements of opinion. These four hypotheses were to be tested with regard, first, to the immediately aroused like or dislike of the content, and secondly, to the subjects' general orientation. In the detailed analysis of each statement necessitated by marking for comprehension, two Liberal statements 71N and 67S, were found to be ambiguous in attitude, i.e. they could be regarded as anti-Liberal also. The results for these two statements will be dealt with separately.

(i) *Immediate attitude and comprehension*

The seven point scales of liking to dislike were correlated with the comprehension scores for every statement. Intelligence and political knowledge were held constant by partial correlation. For this, all test scores were converted into standard scores and divided into seven groups, from best to worst, by 0.7 σ units ranging equally about zero from +2.5 to -2.5. (This was found to be the best approximation to a normal distribution. For statistical details, see Friedmann, 1952.)

None of the partial correlations for the twenty-two unequivocal statements were significant by the criterion—twice the standard error of the correlation. The distribution of the correlations was of a random order tending to zero. Since no positive or negative linear relationship, and no tendency in either direction, was found between immediately expressed liking and comprehension when intelligence and knowledge were held constant, the first two hypotheses must be regarded as disconfirmed for our material.

The third possible relationship between immediate liking and comprehension is a non-linear one. The correlations between liking and comprehension were, consequently, tested for linearity. With $k-2$ degrees of freedom (i.e. 5 for these cases) χ^2 is significant at the 0.05 level if it has a value above 11.07. (Fisher, 1950, table III). For twenty of the twenty-two correlations the deviations were not significant by this criterion, while two were significant at the 0.05 level. According to Wilkinson (1951), and Sakoda, Cohen & Beall (1954), up to three results, significant at the 0.05 level, can occur by chance in a sample of twenty-two. The two significant deviations can, therefore, best be regarded as chance results. This is supported by the internal evidence: a comparison between the scattergrams for comprehension and attitude, and those of knowledge and intelligence and attitude revealed no differences. It is, therefore, likely that for these two statements the most intelligent and knowledgeable subjects happened to be distributed in a particular way with regard to attitude. The scattergrams did not, in any case, show any tendency to be inversely U-shaped, as would be expected if neutral attitudes were connected with better, and like and dislike with worse, comprehension.

(ii) *General political attitude and comprehension*

Subjects were divided into three groups (i.e. favouring, opposing, and neutral) for every statement, on the basis of the attitude test and the opinions expressed in the scripts. The differences between the scores of the three groups were tested by Fisher's t -formula to determine whether there was any consistent or significant superiority. Any one of the first three hypotheses would be supported only if one of the three attitude groups showed significant superiorities in comprehension which could not be explained by similar advantages in intelligence or knowledge scores alone.

Among the sixty-six comparisons between comprehension scores, there were two significant results at the 0.05 level. Statistical considerations make it likely that these two results should be regarded as due to chance. The internal evidence supports this, since the groups that scored significantly more for comprehension also had superior intelligence and knowledge scores. Similar arguments also apply to the four significant results obtained among the 132 comparisons between intelligence and knowledge scores. The obtained differences for comprehension showed no consistent tendency in the direction of any one of the three attitude groups.

(iii) *Results for the ambiguous statements*

The sole criterion for dividing the subjects into favouring and opposing groups for the two ambiguous statements was the subjects' agreement or disagreement with the attitude they had taken the statement to express.

For the difficult statement 67 S, there was no difference between the favouring, opposing and neutral groups for comprehension, intelligence or knowledge, and the partial correlation between liking and comprehension was not significant.

There was a significant partial correlation (-0.3535) for the easy statement 71 N. The correlation between attitude and comprehension departed significantly from linearity (χ^2 from $\eta CA = 12.65$; χ^2 from $\eta AC = 10.62$), and the characteristics of the scattergram of attitude and comprehension were different from those of attitude and intelligence and knowledge. The t -tests showed the opposing group to have a significantly higher

comprehension score than the favouring or neutral groups without similar superiorities in intelligence and knowledge. The internal evidence, then, seems to suggest that there may be a genuine connexion between dislike and comprehension for this statement. If, however, the results for 71 N are considered in conjunction with those for the other twenty-three statements (i.e. as one significant correlation in twenty-four and a total of three significant *t*-test results in seventy-two comparisons for comprehension), both findings for 71 N can be regarded as chance results on statistical grounds. The fact that the other ambiguous statement, 67 S, did not give the same results as 71 N makes this a more likely explanation.

(iv) *Supplementary experiment*

Subjects in this experiment divided well into two attitude groups: right-wing and left-wing (i.e. mainly Conservative and Labour). These were treated as favouring or opposing groups according to whether the statement was of Conservative or Labour origin. There were no significant differences between the two groups on either the N.I.I.P. test or the political knowledge test.

Fisher's *t* was calculated for differences in comprehension on the Conservative statements 52 C, 36 F, 54 F, and on the Labour statements 63 C and 6 F. The differences were not significant. This confirms the results of the main experiment.

IX. DISCUSSION

The experimental results do not support the view that comprehension of statements depends on the direction of the subjects' attitudes to the opinions expressed in them. This was the case for easy, average, and difficult sentences, despite the fact that the material was designed to, and did, arouse strong feelings on political matters. Many subjects expressed themselves most forcibly in the scripts. A qualitative analysis of the scripts corroborated the findings. Making sense of a sentence was seen to be like other attempts at solving problems. Subjects asked themselves questions and tried to answer these, or they tried out various meanings of each term separately. A number of subjects commented that they had used such a method. In other cases, it was clear from the scripts that this had been done. Correct interpretations were those by which the meanings given to each term fitted in with each other, and so made the sentence intelligible. Mistakes arose mainly when subjects got stuck with an interpretation of a term which—although possible—was not the correct one in the context. Instead of trying out another meaning, they retained their first interpretation, and either considered the statement to contain a contradiction, or they changed the remainder of the sentence to fit in with this sense of the word, or they omitted the term altogether while still giving some meaning to the sentence. The resultant misinterpretations could be classified as follows: (a) Substituting a familiar idea, (b) substituting a 'pet' idea, (c) believing an idea to be attacked which was, in fact, supported or not mentioned in the statement, (d) unwarranted elaborations, (e) omissions of essential parts of the statement. As in other experiments on attitudes and cognitive processes, the misinterpretations showed the subjects' bias. The occasion for the mistakes was, however, almost invariably a word or phrase which most subjects—whatever their political bias—had found difficult. Thus, the misinterpretations of a sentence by a Conservative, a Liberal, and a Socialist would be different in content, but the words which had been misconstrued or omitted were the same.

The subjects themselves also cited the construing of terms as their major difficulty.

They complained of (a) the vagueness of terms, (b) their level of generality, (c) the number of such vague or general terms in a sentence, (d) the number of subordinate clauses, (e) inability to give examples, and (f) illogicalities and contradictions in the statements.

The material suggests that mistakes mainly arose from lack of knowledge or skill in manipulating words. When an objective difficulty—too many vague terms or some other intellectual lacunae—prevented a correct interpretation, the subject fastened on whatever items were familiar and elaborated material which, to him, was associated with these. Thus, the *content* of mistakes frequently revealed emotionally associated material; the *number* and *kind of occasions* for mistakes—by which efficiency is measured—depended in the main on intellectual factors and seemed to be independent of direction of the subjects' political attitudes. In other words, a distinction must be made between the kind of occasions from which mistakes arise and the content of mistakes.

Caution should be used in generalizing these results, since the number of subjects was relatively small and they were drawn mainly from an undergraduate and graduate population. The findings suggest, however, that careful scrutiny is needed of statements which include social attitudes among the major determinants of efficiency of comprehension.

I am indebted to Prof. C. A. Mace for his help and guidance in supervising the investigation.

REFERENCES

- BARTLETT, F. C. (1932). *Remembering*. London: Cambridge University Press.
- BAUMGARTEN-TRAMER, F. (1941). Zur Psychologie des Verstehens und der Verständigung. *Gesundh. Wohlf.* no. 2, 61–4.
- COOPER, E. & JAHODA, M. (1947). The evasion of propaganda. *J. Psychol.*, **23**, 15–25.
- DAVIS, F. B. (1942). Fundamental factors of comprehension in reading. *Psychol. Bull.* **39**, 499–500.
- DAVIS, F. B. (1944). Fundamental factors of comprehension in reading. *Psychometrika*, **9**, 195–97.
- DOOB, L. W. (1947). The behavior of attitudes. *Psychol. Rev.* **54**, 135–56.
- EDWARDS, A. L. (1941). Political frames of reference as a factor influencing recognition. *J. Abnorm. (Soc.) Psychol.* **36**, 34–50.
- FISHER, R. A. (1950). *Statistical Methods for Research Workers*. Edinburgh: Oliver and Boyd.
- FRIEDMANN, S. (1952). The influence of attitudes on the understanding of verbal material. Ph.D. Thesis, London 1952.
- HIPPIUS, R. (1936). Gefühl und Denken, Ber. 15. *Kongr. Dtsch. Ges. Psychol.* Jena, 52–65.
- HODGES, H. A. (1944). *Wilhelm Dilthey*. London: Routledge and Kegan Paul.
- LEVINE, R. & MURPHY, G. (1943). The learning and forgetting of controversial material. *J. Abnorm. (Soc.) Psychol.* **38**, 507–17.
- MARUM, O. (1936). Analyse des Verstehens und seiner Bedingungen. *Arch. ges. Psychol.* **95**, 99–115.
- MASON, H. M. & GARRISON B. K. (1951). Intelligibility of spoken messages—liked and disliked. *J. Abnorm. Soc. Psychol.* **46**, 100–3.
- RICHARDS, I. A. (1930). *Practical Criticism*. London.
- RYLE, G. (1949). *The Concept of Mind*. London: Hutchinson.
- SAKODA, J. M., COHEN, B. H. & BEALL, G. (1954). Tests of significance for a series of statistical tests. *Psychol. Bull.* **51**, 172–5.
- SEELEMAN, V. (1940). The influence of attitudes upon remembering pictorial material. *Arch. Psychol.* no. 258.
- SHERIF, M. & CANTRIL, H. (1945). The psychology of attitudes, I. *Psychol. Rev.* **52**, 295–319.
- SHERIF, M. & CANTRIL, H. (1946). The psychology of attitudes, II. *Psychol. Rev.* **53**, 1–24.
- VERNON, P. E. (1950). The intelligibility of broadcasts. Audience research department, British Broadcasting Corporation.
- WILKINSON, B. A. (1951). A statistical consideration in psychological research. *Psychol. Bull.* **48**, 156–8.

(Manuscript received 29 April 1957)

TIME FOR READING AND MEMORY

By E. C. POULTON

Medical Research Council Applied Psychology Research Unit, Cambridge

This paper on silent reading is concerned with the rate of comprehension, selection while reading, and the rigidity of pacing while reading. Subjects studied 144 short statements under one of three degrees of pacing: (a) a time was set for studying the whole series of statements (whole condition), (b) each complete statement was presented in turn by itself for a set time (statement condition), and (c) part only of a single statement was presented at a time, and there was a period after each statement when no print was visible (segment condition). In different subconditions 1.0 min. was allowed for an average of every 293, 146, 73 or 37 words. In one variation of condition (a) the subject was instructed to pay attention only to the 18% of statements connected with geography (selection condition). In another variation, the time for study was fixed at 3.0 min., and the number of statements was reduced accordingly (shortened condition).

After reading, the subject had first to estimate from memory the percentage of statements which could be classified under certain specified topics. He was then given a test of memory for seventy-two of the statements. Half of them had to be completed (recall), and each of the remaining half had to be discriminated from a paired statement which resembled it in one of six standard ways (recognition). An identical test of memory for the remaining seventy-two statements was normally given unexpectedly 1 week later. A control group of subjects took these tests without having read the statements.

In the whole condition when 1.0 min. was allowed for every 293 or 146 words, most of the subjects reported that they had read practically all the statements once and only once. Reading at about 146 words per minute in this condition was found to give significantly better comprehension of the individual statements than reading at about 293 words per minute. In contrast, in this whole condition when 1.0 min. was allowed for every 73 or 37 words, the subjects reported that they had re-read some or all of the statements at least once. Between the rates of presentation of 37 and 293 words per minute, substance memory varied approximately as the logarithm of the time available per statement. After the 3.0 min. of study in the shortened condition, about the same number of statements could be remembered in substance or recognized whether 293 or only 73 words had had to be studied per minute. Selection tended to improve memory for the selected statements, and to reduce memory for the remainder. But when 293 words had to be studied per minute, only recognition soon after study benefited significantly from selection.

When 1.0 min. was available for every 146 words, the rigid pacing of the segment condition gave significantly better memory soon after study than the loose pacing of the whole condition. But when 1.0 min. was available for every 37 words, pacing had no effect upon memory. There were no significant differences between the degrees of pacing used in the statement and whole conditions.

I. INTRODUCTION

(i) *Speed of reading and comprehension.* The primary aim was to determine how comprehension is affected by altering the speed of reading to the greatest feasible extent. No conclusive experimental answer yet appears to have been given to this question. If the function relating speed of reading to comprehension corresponds to the typical learning curve relating time for study to memory (e.g. Woodworth, 1938, Fig. 6), speed of reading and comprehension would be inversely related. The only exception would occur at speeds of reading which gave the highest score on the test of comprehension which the particular reader was ever likely to obtain, when there would be no correlation. However, the reported correlations between speed of reading and comprehension have often been positive. And in describing the reading-training programme at Iowa State College, in which speed was used as a criterion of improvement but not comprehension, Lauer stated

(1936, p. 655) 'it is quite generally agreed that speed in reading is positively correlated with comprehension'. Some of this evidence will now be outlined, together with the reasons for disregarding it.

First, Tinker (1939) found that with all but the most difficult standardized reading tests, the faster undergraduate reader obtained a higher comprehension score than the slower reader. But this result appears to have been an artifact of the experimental method he used. For in these reading tests comprehension was assessed by multiple-choice questions answered by the reader as he went along. The reader, therefore, presumably adjusted his rate of reading to the difficulty of the questions. The net result was that mistakes were rarely made, except in the difficult passages where more than one interpretation was possible. Thus, with the easy passages the number of questions answered in a given time, which is a criterion of speed, determined the recorded score for comprehension. Speed of reading and comprehension were therefore found to be highly correlated. (Tinker actually took as his criterion of speed the time to complete the alternative version of each test (1940). But he stated that this criterion correlated almost perfectly with the number of questions answered in a given time in the original version.) A modification of this approach, which avoided the difficulty, was used by Stroud & Henderson (1943). They gave multiple-choice tests of recognition *after* the reading. They found a correlation of only about $+0.1$ between speed of reading and success in the tests. But even if the correlation had been higher, no valid deductions could have been made from this correlation to the relationship between speed and comprehension within a single individual. For the experimental design confounded the relationship between speed and comprehension with individual differences in previous knowledge (Poulton, 1957*a*), intelligence, and personality characteristics. The faster readers might have recognized more still if they had read rather more slowly.

A second line of evidence comes from the before and after scores of young adults trained to read faster. Generally a large and statistically significant improvement is found in the speed of reading, together with a smaller and often not significant improvement in comprehension as measured by multiple-choice recognition questions. But again no valid deductions which are relevant to our question can be made from data of this kind, for here the relationship between speed of reading and comprehension has been confounded with the effects of the training.

A third line of evidence is supplied by Perry & Whitlock (1948*b*, fig. on inside of back cover). They used, as an independent criterion of reading ability, the grades achieved in certain elementary history courses at Harvard University (Perry, 1955). They found that the same average grade was achieved by a moderately fast reader with moderate comprehension, a fast reader with poor comprehension, and a slow reader with good comprehension. Thus here the correlation was negative. Unfortunately, they did not ask the slow reader to read faster, nor the fast reader to read more slowly, and determine the changes in comprehension score. Thus these data also do not answer our question.

(ii) *Subsidiary aims*. In everyday life we cannot always ask ourselves: 'how long ought I to spend on this document?' We may have only a limited time available, and the question then becomes: 'how should I distribute my time?' Is it possible to learn as much by reading a part only of the document carefully, as by reading it all more hurriedly? A subsidiary aim was to answer this question, both when the part to be read was selected at random, and when the reader knew what topic to concentrate on.

A second subsidiary aim was to find out whether comprehension was affected by the degree of rigidity of pacing while reading. Excluding the tachistoscope, there are, in general, three degrees of pacing which have been used in training adults to read faster (Glock, 1949). The least rigid form is simply to hand the subject a passage, and to tell him to read it as quickly as he can. In the intermediate form of pacing, some kind of marker descends slowly down the page, and the subject has to keep ahead of it. In the most rigid form, only a part of a line of print can be seen at a time, as in the Harvard reading films (Perry & Whitlock, 1948*a*). Similar (though not identical) degrees of pacing were used in the present experiment.

(iii) *Reading material*. This consisted of a series of short disconnected statements, and was chosen for three reasons. First, comprehension of this type of material should be less adversely affected by increasing the speed of reading than comprehension of a developing argument. For comprehension of an argument is largely 'all or none'; insufficient time to understand any one of its stages means failure to understand the whole properly. Whereas with disconnected statements failure to understand one statement should not appreciably affect the chances of understanding the next statement. Thus, if there were any truth in the view that speed of reading is either positively correlated with comprehension, or at least unrelated to it, it would have a good chance of being confirmed with this type of material. Secondly, memory for each statement could be scored equally and objectively. The experimenter did not have to select the parts of a passage for which memory was to be tested. Thus there would be no question of a subject who selected what was worth remembering, much as the experimenter selected what was to be tested, doing better than a subject who made an unlucky selection. Thirdly, the material was the same as that used in an earlier experiment (Poulton, 1957*a*). Thus it would be possible to relate some of the results of the two experiments.

II. METHODS

The same material was used as in a previous experiment (Poulton, 1957*a*). It is reproduced in full in the Appendix to Poulton (1957*b*). There were four lists, each of thirty-six statements. A rough classification of the statements according to topic is given in the first row of Table 6 below. The statements ranged in length from 3 to 9 words, with an average of 6.1 words. One of the longest was: 'The stern may be the rear of a steamer.' Each statement started with a title, e.g. 'The stern'; and contained two key words, e.g. 'rear' and 'steamer'. Some of the titles contained unfamiliar words, but all the key words were in everyday use. Half the statements in each list were at least approximately true, and the other half were to some degree false. The statements were typed one below the other on two paper tapes 6 in. wide. The typing was double spaced, twelve letter spaces to the inch. In all conditions except a shortened condition one tape carried lists 1 and 2 without a break between them, and the other tape carried lists 3 and 4. In the shortened condition the number of statements on each tape was reduced (see below).

(i) *Reading*. The experiment was carried out in three parts: reading, immediate test of memory, and delayed test of memory. In the first part, each subject was tested individually. He studied the statements with the knowledge that he would be asked questions on them afterwards, although the nature of the questions was not revealed. There was a break of about 1.0 min. between the presentation of the two paper tapes. The eighteen subconditions of study are listed in Table 1. Each subcondition was performed by a separate group of eight subjects. The reading was always preceded by a suitable practice on eighteen statements, which allowed the subject to learn his particular procedure.

In a *statement* condition the statements were presented automatically in succession, through a horizontal slit in a screen. Each statement was shown once only for 10.0, 5.0, 2.5 or 1.25 sec., depending upon the subcondition. At the end of the permitted time, the next statement was moved smartly into position by a solenoid. The presentation rate of 1.25 sec. per statement required reading at about 293 words per minute.

In a *segment* condition a clear celluloid disk rotated behind the screen which contained the horizontal

Table 1. *Conditions of study*

Time for study		Time of exposure (sec.)					
Sec./statement	Words/min.	Segment	Statement	Whole	Selection	Shortened	Control
10.0	36.6 (37)	1.05	10.0	720	720	90	—
5.0	73.2 (73)	0.95	5.0	360	360	90	—
2.5	146.4 (146)	0.80	2.5	180	180	90	—
1.25	292.8 (293)	*	1.2	90	90	*	—
0.0	∞	—	—	—	—	—	0

Note. Each subcondition was performed by a separate group of eight subjects, except the control which was performed by forty-eight subjects.

* This subcondition was not used.

slit. The disk carried a black paper mask with a spiral window in it. Thus, the subject saw at a time only a segment of the typewritten line of a fixed size. This segment moved smoothly across the paper tape, so that new letters appeared on the right as letters disappeared on the left. As soon as the window moved off the tape at the right-hand edge, and before it appeared again on the left, the next statement was placed in position. The apparatus will be described in greater detail in a subsequent paper. In the present experiment when 10.0 sec. was allowed per statement, the size of the visible segment was nine letter spaces. A new set of letter spaces appeared every 1.05 sec. Because the average length of a statement was only 2.5 in., while the width of the paper tape was 6.0 in., no letters were visible on average for 5.6 sec. out of the 10 sec. allowed per statement. During the time that the subject was actually reading, he had therefore to read at an average speed of 84 words per minute. At 5.0 sec. per statement, nineteen letter spaces could be seen at a time, a new set appeared every 0.95 sec., and no letters were visible on average for exactly half the time. The average speed of actual reading was therefore 146 words per minute. At 2.5 sec. per statement, thirty-seven letter spaces could be seen at a time, a new set appeared every 0.80 sec., and no letters were visible on average for 1.05 sec. out of the 2.5 sec. available, so that the average speed of actual reading was 252 words per minute. (No presentation rate of 1.25 sec. per statement was used, because in a pilot experiment subjects reported that they could not see each statement for long enough to read it, even with a moving window the full width of the paper tape.)

In a *whole* condition the subject was handed a complete paper tape at a time, and told to study it for the total time permitted for the seventy-two statements. The experimenter called out when one-, two-, and three-quarters of the time had passed, and the ends of the quarters were marked on the tape. The subject was told that at a quarter time he must leave the first quarter of the statements, if he had not done so already; at half time he must leave the second quarter, and so on. If he was ahead of schedule, he was told to employ whatever time was left over in additional study. When time was up, he had to report how many times he had read each statement. The same four rates of presentation were used as in the statement condition.

In a *selection* condition the subject had to study only the geographical statements. These were defined as statements 'concerned with other countries or parts of this country'. The procedure was otherwise the same as in the whole condition.

In a *shortened* condition the subject was allowed only 1.5 min. for study in each half of the period of reading. Special shortened paper tapes had therefore to be prepared. No presentation rate of 1.25 sec. per statement was used, since this subcondition would have been identical with the corresponding whole subcondition. When 2.5 sec. were available per statement, four subjects were given lists 1 and 2, respectively, for the two halves of the period of reading. The other four subjects were given lists 3 and 4. The order of studying the lists was always counterbalanced for different subjects in all subconditions. When 5.0 sec. were available per statement, two subjects were given only the first halves of lists 1 and 2, respectively, for the two halves of the period of reading, and two subjects were given the second halves of lists 1 and 2. Lists 3 and 4 were divided in similar fashion between the other four subjects. When 10 sec. were available per statement, half the statements in each half list were not used at all. The design was otherwise similar to that used at 5 sec. per statement. The blocks of statements selected and rejected were equated for difficulty as judged by the results of the other experimental conditions.

(ii) *Tests of memory.* The second part of the experiment followed directly after the first. The subject was taken to another room, where one or more other subjects might already be working. First, he had to fill in his personal details on a form. Secondly, he had to estimate the percentage of the statements which fell

into the following five categories: (a) The navy and life at sea. (b) History, both old and contemporary. (c) Geography (places and people in other countries, and in other parts of this country). (d) Science including physics, engineering, biology and medicine. (e) Other subjects.' To help him, he was given two examples of each type, taken from the practice statements (see Appendix to Poulton, 1957*b*). The order of the first four topics on the form was different for different groups of subjects.

Then came a test of recall, identical with that given in the previous experiment (Poulton, 1957*a*). The titles of one list of statements were presented on an otherwise blank sheet, and the subject had to complete as many of the statements as he could. In the shortened condition, the list was selected from the pair of lists of which the subject had read at least some of the statements, and he was asked to guess the answers to any statements which he had not seen previously.

Finally came a test of recognition, which was again identical with that of the previous experiment. The statements of a second list were presented in full. (In the shortened condition this list was the second of the pair of lists of which at least some of the statements had been read.) Each statement had a similar statement typed either above or below it, from which it had to be distinguished. The alternative version had the same title as the original. Half the alternative versions had one different key word, and the other half had both key words different. Each of these subgroups was divided into three, according to meaning. The meaning of the alternative was either more or less the same as the original, in some sense the opposite, or as far as possible unrelated (see Appendix to Poulton, 1957*b*). If the subject was not sure which of a pair to select, he was made to guess. Unlimited time was available for all these tests of memory.

A delayed test of memory was given 1 week later, except in the shortened condition. It came as a surprise to all subjects. Six subjects were tested at a time. The test employed the other two of the four lists of statements read. Recall of the individual statements of one list was tested first, followed by recognition of the statements of the other list. The two tests were similar to the corresponding tests of the previous week.

(iii) *Balanced design.* The order of reading the two paper tapes, the times allowed for reading, the order on the form of the four specific topics in the first test of memory, and whether the lists of statements had to be recalled or recognized, were all balanced in a multifactorial design for each condition. Whether memory for a list was tested immediately or after a week was also balanced in the multifactorial design, except in the shortened condition which had no delayed test of memory.

(iv) *Control group.* A group of forty-eight subjects had to take the immediate tests of memory without having read the statements. The procedure was similar to that used for the control group in the previous experiment (Poulton, 1957*a*) but, in addition, the subjects first made estimates of the percentage of statements which could be classified under each of the five headings. The order on the form of the four specific topics was again balanced for different subgroups. On each list of statements twelve subjects then took the test of 'recall'; another twelve subjects took the test of 'recognition', after a test of 'recall' on one of the other lists.

(v) *Subjects.* The 192 subjects were all naval ratings drawn from the same population as the subjects of the previous experiment (Poulton, 1957*a*). Their ages ranged from 17 to 36, with a median of 21. Their scores on intelligence test AH4 (Heim, 1955) ranged from 31 to 121, with a median of 70. Most had left school at about 15, and had been apprentices or had taken miscellaneous jobs before joining the navy. The experimental subgroups were selected to be of approximately similar composition.

(vi) *Scoring.* The same methods of scoring were used as in the previous experiment (Poulton, 1957*a*). Correct literal recall of a statement required memory for the two key words, but not the exact verbal structure of the original. Correct substance recall required memory for the meaning of at least one of the two key words, expressed in a form which did not alter too grossly the general sense of the other key word.

In order to assess the effect upon memory for geographical and non-geographical statements of selective study of the geographical statements (Table 5), sixteen of the 144 statements were classified as unambiguously geographical (see Appendix to Poulton, 1957*b*). Each of these statements contained the name of a place or country. Another ninety-nine statements were classified as definitely non-geographical. These two forms of statement were divided more or less equally between the four lists. The remaining twenty-nine statements were not considered in making this comparison, as they could be taken to be borderline cases.

In order to assess the effect upon the memory scores of the possible interaction of time for study with previous knowledge, three sets of six statements each were selected from each list (see Appendix to Poulton, 1957*b*). The criteria for selection were respectively: (a) the six statements in each list which had the highest indices of general knowledge in the previous experiment (Poulton, 1957*a*); (b) the six statements with an unfamiliar word in the title which received the greatest number of 'no idea' ratings in the

previous experiment; and (c) the six statements in each list without an unfamiliar word in the title which had the lowest indices of general knowledge.

(vii) *Calculations.* As in the previous paper (Poulton, 1957*a*), a recognition score was computed by doubling the number of errors in the test of recognition, and subtracting this product from the number of pairs of statements presented. Wherever possible the significance of the difference of each pair of means was assessed separately (*a*) against the variability between subjects, and (*b*) against the variability between statements. The level of the less significant of the two tests was taken. This test was generally (*a*). Two-tailed tests were used except where stated. As compared with more conventional methods, this method tends to underestimate significance. For the data in Tables 3 and 6, only tests of type (*a*) could be used.

A table was sometimes found to contain few significant differences. In this case the total number of comparisons made was taken into account in assessing the importance of the results, after the method described by Sakoda, Cohen & Beall (1954). If this overall assessment was not significant, the individual differences which were found to be significant have not always been mentioned in the results, unless they were to have been expected on other grounds.

The significance of the difference in position between the two top curves in Fig. 1 was determined by comparing each of the three corresponding points which did not coincide, using *t*-tests. The three *P*'s were then combined by Fisher's χ^2 -method (Fisher, 1948, pp. 99-101). The lowest point was common to both curves, and so could not be used.

The standard deviations in Table 3 were calculated from the two verbal reports of each subject, one for each half of the period of reading. Thus, they cannot be used to estimate the significance of the differences between conditions, since they include variability within subjects as well as the variability between subjects.

The scores in the lower half of Table 4 (and of the lower broken curve in Fig. 1) were computed from the scores in the upper half of the table (or of the upper broken curve), by allowing the same proportion of correct guesses on unseen statements as was achieved by the forty-eight control subjects. The data from the shortened condition could not be used as they stood, because the group of subjects who studied at 146 words per minute (2.5 sec. per statement) did not do any guessing; they had simply to recall the statements of one of the two lists which they had read, and to recognize the statements of the other list (see above). The guessing of the two groups of subjects who studied at 73 and 37 words per minute, respectively, was very similar to the guessing of the control group. The significances of the differences in the lower half of Table 4 were calculated from the actual scores in the shortened condition.

The significance of the influence of the order of the topics on the form upon the proportion of statements classified under each topic (Result (vii)) was estimated by analysis of variance.

III. RESULTS

(i) *Time for study and memory.* The effect of the time allowed for study of the 144 statements upon memory is shown in Table 2 for the three degrees of pacing. The scores for the guessing of the control subjects are given in the first column. In the last column are the scores from a condition of the previous experiment (Poulton, 1957*a*), in which one statement was heard every 10 sec. By all the criteria of memory used, the amount remembered increased significantly as the time allowed per statement was increased. For all degrees of pacing combined, memory after a week was still better than guesswork (no study), except for recall after 293 words had been presented per min. Substance memory was actually a little below the level of chance in this latter case, because the subject would not always guess when he had forgotten a statement.

Fig. 1 shows graphically the relationship between substance memory and the number of words to be studied per minute, in both the whole and the shortened conditions. If we consider only memory soon after study for the statements actually studied, in both conditions (between 293 and 37 words to be studied per minute) substance memory varied inversely approximately as the logarithm of the number of words to be studied per minute. However, the complete theoretical curves are presumably S-shaped, and this tendency is apparent in Fig. 1. (The function for the shortened condition lies above the

Table 2. *Effect upon memory for 144 statements of time for study and degree of pacing*

Criterion of memory	Words to be studied/min.						
	No study (control) (%)	293			146		
		State-ment (%)	Whole (%)	Segment (%)	State-ment (%)	Whole (%)	
Memory soon after study							
Two key words reproduced	0.9*	10	13	27†	14	17‡	
Meaning more or less reproduced	8.7*	23	22	48†	37‡	32‡	
Recognition score	-1*	20	22	66	67‡	44‡¶	
Memory after 1 week							
Two key words reproduced	0.9*	1.4	1.0	3.1	3.1	3.5‡	
Meaning more or less reproduced	8.7*	7.3	7.3	15	10	13‡	
Recognition score	-1*	2	22††	38	29‡	28	
Criterion of memory	Words to be studied/min.						
	No study (control) (%)	73			37		
		Segment (%)	State-ment (%)	Whole (%)	Segment (%)	State-ment (%)	Whole (%)
Memory soon after study							
Two key words reproduced	0.9*	35	32§	26§	32	32	28
Meaning more or less reproduced	8.7*	59	52§	50§	55	60	56
Recognition score	-1*	74	74	77§	80**	83	69
Memory after 1 week							
Two key words reproduced	0.9*	5.2	8.0	7.3§	3.8	5.2	6.3
Meaning more or less reproduced	8.7*	18	18§	19§	18	18	16
Recognition score	-1*	29	54§††	43§	42	59	57

Note. The segment condition was the most rigidly paced, and the whole condition was the least rigidly paced. A negative recognition score is below the level of chance.

* For all degrees of pacing combined, no study different from all rates of study at the 0.05 level of confidence with a one-tailed test or better, except 293 words per minute on the two measures of recall after 1 week.

† Segment different from both statement and whole at the 0.05 level or better.

‡ 293-146, $P=0.05$ with a one-tailed test or better.

§ 146-73, $P=0.05$ with a one-tailed test or better.

|| Listening—statement, $P=0.05$ or better.

¶ Whole different from both segment and statement at the 0.01 level.

** 146-37, $P=0.05$.

†† Statement—whole, $P=0.05$.

‡‡ Segment—statement, $P=0.05$.

function for the whole condition, but the difference was not significant. The difference was presumably due to the fewer statements which had to be memorized in the shortened condition at the slower rates of study. Thus when only 37 words had to be studied per min., there were only eighteen statements to be memorized in the shortened condition, as compared with the full 144 statements in the whole condition.)

(ii) *Reported speed of reading.* The mean numbers of times the lists were reported to have been gone over in the loosely paced conditions are shown in Table 3. The standard deviations of the means for the two halves of each period of reading are also given (see Calculations). Except when 293 words had to be studied per minute, the lists were gone over least frequently in the whole condition, and most frequently in the selection condition.

In the whole condition the average reported speeds of reading were thus 299, 170, 126 and 133 words per minute respectively, for the four subconditions. When 293 words had to be studied per minute in the whole condition, no subject had time over at the end in which to re-read more than a very few of the statements, and two of the eight subjects

failed to read quite all of them. (The memory scores of these two subjects were about the same as the scores of the remainder of the subjects.) Thus in this subcondition the time allowed for study in words per minute corresponded fairly closely to the reported average speed of reading of each subject.

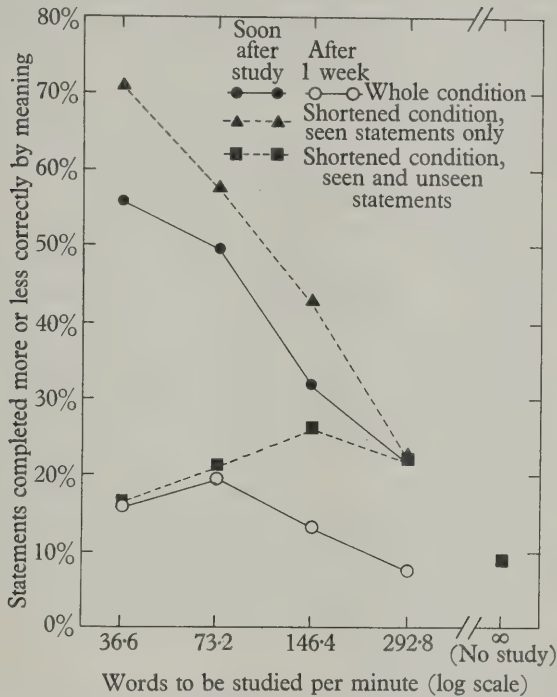


Fig. 1. The relationship between words to be studied per minute and substance memory in the whole and shortened conditions. Each point on a curve is based upon data from a separate group of eight subjects. The points on the lower broken curve were computed from the corresponding points on the upper broken curve, by allowing 8.7% correct responses for unstudied statements. This is the value for no study which was given by the control group of forty-eight subjects.

Table 3. *Number of times lists were gone over*

Condition	Words to be studied/min.							
	293		146		73		37	
	Mean	S.D.	Mean*	S.D.	Mean†	S.D.	Mean	S.D.
Whole	1.02	0.048	1.16	0.23	1.72	0.56	3.63	1.01
Shortened	—	—	1.44	0.39	2.41	0.71	4.16	1.38
Selection	0.97	0.042	1.70	0.64	2.73	0.43	4.20	1.74

* Whole different from selection at the 0.05 level of confidence.

† Whole different from shortened at the 0.05 level, and from selection at the 0.001 level.

In the whole condition when 146 words had to be studied per minute, all the subjects finished reading the statements, and six of the subjects had time to re-read only a very few of them. The reported average speed of reading of these six subjects was about 153 words per minute. The two remaining subjects re-read just under and just over half the statements, respectively, but their memory scores were about average. Thus, the memory

scores for this subcondition can be taken as reasonably representative of subjects actually reading at about the required rate of study (146 words per minute).

In contrast, in the whole condition when seventy-three words or less had to be studied per minute, the time allowed for study no longer corresponded to the average reported speed of reading. When these long times were available, the rigid pacing of the segment condition was the only method of ensuring that each statement was read once and only once.

(iii) *Reported speed of reading and memory.* We have just seen that, in the whole condition when 293 or 146 words had to be studied per minute, either the reported speed of reading in words per minute corresponded fairly closely to the time allowed for study or at least the memory scores were reasonably representative of subjects reading at about the set speed. Table 2 shows that, in this condition, memory was significantly better when 146 words had to be studied per minute, than when 293 words had to be studied per minute, by every criterion except delayed recognition. Thus, we may conclude that the amount remembered increased significantly when the average reported speed of reading decreased from about 293 to about 146 words per minute.

The effect of reducing the average speed of reading below about 146 words per minute could only be determined in the segment condition. The rigid pacing in this condition ensured that the subject read each statement once only at a controlled speed. The range of controlled reading speeds ran from an average of 252 words per minute at the presentation rate of 146 words per minute, to 84 words per minute at the presentation rate of 37 words per minute (see Method). Table 2 shows that, in this segment condition recognition soon after study was significantly better after reading at the slow speed than after reading at the fast speed. The other criteria of memory showed similar trends, but they were not significant statistically.

(iv) *Degree of pacing and memory.* Table 2 also shows the effect upon memory of the degree of pacing imposed during the reading. Soon after 146 words had been presented per minute, the rigid pacing of the segment condition gave significantly better memory scores than the loose pacing of the whole condition. Yet the distributions of scores on the intelligence test AH4 (Heim, 1955) were almost identical for the two groups of subjects. The segment condition also gave better recall than the whole condition soon after 73 words had been presented per minute, but the differences were not significant.

There were no obvious differences in memory between the three degrees of pacing soon after only 37 words had been presented per minute. The only significant difference soon after study between the statement and whole conditions was in recognition after 146 words had been presented per minute. This difference could have been due to chance since in twelve comparisons one significance at the 0.01 level has a probability of occurrence greater than 0.1 (Sakoda *et al.* 1954, fig. 2). Similarly, the two significant differences in memory after a week between the three degrees of pacing could have been due to chance. For in twenty-one comparisons, two significances at the 0.05 level have a probability of occurrence of about 0.25 (Sakoda *et al.* 1954, fig. 1).

(v) *Distribution of study and memory.* Table 4 shows the effect upon memory soon after study in the shortened condition of the amount to be studied in the two periods of 1.5 min each. The upper half of the table gives the scores for the statements actually studied. As in Table 2, memory for the statements studied increased significantly as the time allowed per statement was increased. The lower half of Table 4 gives computed scores for all 14

statements, whether studied or not (see Calculations). Both for substance memory and for recognition, the scores for the whole 144 statements were about the same whether an attempt had been made to study all 144 statements (293 words to be studied per minute), or whether only one-quarter of the statements had been studied in the time (73 words to be studied per minute). There was a suggestion that it may actually have been better to have concentrated on only half the statements (146 words to be studied per minute), than to have attempted to study them all. However, when recall of the actual words was the criterion, memory soon after study was rather better when all the statements had been looked at.

Table 4. *Effect of amount to be studied in 3 min. upon memory soon afterwards*

Criterion of memory	No study (control) (%)	Words to be studied/min.			
		293 (%)	146 (%)	73 (%)	37 (%)
Statements actually studied					
Two key words reproduced	—	13*	18	30	39
Meaning more or less reproduced	—	22	43†	58	71
Recognition score	—	22	60‡	85	84
Computed scores for all statements (seen and unseen)					
Two key words reproduced	0.9	13	10	8.1	5.6
Meaning more or less reproduced	8.7	22	26	21	17§
Recognition score	-1	22	30	21	10§

Note. A negative recognition score is below the level of chance.

* 293 different from 73 at the 0.05 level of confidence.

† 146 different from 293 at the 0.01 level and from 37 at the 0.05 level.

‡ 146 different from both 293 and 73 at the 0.01 level.

§ 146 different from 37 at the 0.02 level or better.

(vi) *Selective study and memory.* Table 5 compares the selection condition with the whole condition on memory soon after study. Soon after 293 words had had to be studied per minute, it was only recognition of the statements to be selected which gave a significant difference in favour of selection. Recall of the statements to be selected was no better in the selection condition than in the whole condition. And on the statements not to be selected, selection resulted in significantly worse recall, and somewhat worse recognition, than simple study. In contrast, soon after only 37 words had had to be studied per minute, selection produced considerably better recall of the statements to be selected than full study, and not much worse memory of the statements not to be selected.

After a week, there were only two differences in memory for the statements to be selected which were significant at the 0.05 level. Both differences favoured selection, but neither occurred after 293 words had to be studied per minute. They could have been due to chance, since for twelve comparisons the combined $P=0.12$ (Sakoda *et al.* 1954, fig. 1). There were also two differences in memory after a week for the statements not to be selected which were significant at the 0.02 level. Both differences favoured the whole condition.

(vii) *Classification from memory soon after study.* The mean estimates of the percentage of statements classified under the different headings are shown in Table 6. The complete set of differences between the various times allowed for study, and between the three degrees of pacing, could have been due to chance (Sakoda *et al.* 1954). The data for the four different times allowed for study, and for the statement and whole conditions, have

Table 5. *Effect of selective study upon memory soon afterwards*

Criterion of memory	Words to be studied/min.								
	No study (control) (%)	293		146		73		37	
		Selection (%)	Whole (%)	Selection (%)	Whole (%)	Selection (%)	Whole (%)	Selection (%)	Whole (%)
Statements to be selected									
Two key words reproduced	2.6	13	13	28	19	56	25*	75	25†
Meaning more or less reproduced	10	19	31	50	28*	63	44	81	50†
Recognition score	-11	75	19‡	62	62	100	87	94	75
Statements not to be selected									
Two key words reproduced	0.6	1.5	14†	4.5	19†	21	26	18	29
Meaning more or less reproduced	7.2	5.1	21†	16	34†	35	51	41	56
Recognition score	-2	12	25	24	41	41	74*	66	70

Note. A negative recognition score is below the level of chance.

* Selection—whole $P=0.05$.

† Selection—whole $P=0.01$ or better.

‡ Selection—whole $P=0.02$.

therefore been pooled. The data for the segment and shortened conditions have not been included, because they were not performed at the fastest rate of presentation (see Table 1). The approximate actual percentages in the top row of the table were calculated from the experimenter's own classification (see Appendix to Poulton, 1957*b*). The bottom row gives the theoretical estimates if all five categories had been equally probable. For each heading in Table 6, the value for the combined statement and whole condition lies on the same side of twenty as the actual percentage. The actual percentages for navy and other topics are the two furthest from twenty, and in these two cases the differences between the combined statement and whole conditions and the control condition (no study) are highly significant. Selection gave similar estimates to the combined statement and whole conditions.

Table 6. *Classification of statement soon after study*

Condition or basis of assessment	Mean percentage judged to be concerned with				
	Navy*	History	Geography	Science†	Other topics*
Approximate actual percentage	7	17	18	26	32
Combined statement and whole conditions	11.6	18.7	19.8	22.8	27.1
Selection condition	10.7	20.1	22.2	19.8	27.2
Control condition (no study)‡	18.3	18.2	21.6	25.8	16.1
Percentage if all categories were equally probable	20	20	20	20	20

* Control different from both combined statement and whole, and from selection, at the 0.001 level of confidence.

† Control different from selection at the 0.02 level.

‡ Science different from all others at the 0.05 level or better; geography different from all others at the 0.05 level with one-tailed test or better.

Two differences which are not shown in Table 6 are in keeping with the results in Table 2. First, for the combined statement and whole conditions, the mean for navy was 13.0 after 293 or 146 words had to be studied per minute, as compared with a mean of 10.5 after 76 or 37 words had to be studied per minute ($P=0.05$). And secondly, after 146 words had to be studied per minute, the mean for navy was 13.4 in the statement condition, as compared with 7.9 in the segment condition ($P=0.05$ with a one-tailed test). The navy was the only specified topic upon which significant differences were to have been

expected, for Table 6 shows that only with this topic was there a large discrepancy between the actual proportion and the guessed estimates of the control subjects.

The control subjects had to make their estimates without any knowledge of the nature of the statements. Table 6 shows that they tended to be most generous in their estimates of Science and Geography, which were the two categories with the longest descriptions (see Method). Both the control and the experimental subjects tended to give extra-generous treatment to the first two topics listed on the form, whichever they were, as compared with the other two specific topics listed ($P=0.05$ or better).

(viii) *Other points*. We also investigated the effect of previous knowledge upon the relationship of the time allowed for study to memory for the individual statements. The data from the statement and whole conditions were combined, since there were no significant differences between these two conditions which could not be attributed to chance (Result (iv)). The difference between the rates of presentation of 293 and 37 words per minute was found to be significant ($P=0.05$ a one-tailed test or better) by all the criteria of memory soon after study, and also by the criterion of delayed recognition, for three degrees of previous knowledge. These three degrees were represented by: (a) statements about whose truth the subject was likely to have been certain and correct (see Scoring); (b) statements which contained an unfamiliar word in the title; and (c) other statements about whose correctness he was likely to have had no idea. The statements in category (a) tended to be remembered best by all the criteria of memory, and to show the greatest rate of improvement in memory soon after study as a longer time was allowed for study. The statements in category (b) tended to be remembered least well, and to show the smallest rate of improvement in memory soon after study as the time for study was increased.

For the statement and whole conditions combined, the difference between the rates of presentation of 293 and 37 words per minute was also significant for both the true and the false statements by all but one of the criteria of memory used. The only exception was delayed memory of the actual words used in the false statements, and in this case the difference between 293 and 73 words per minute was significant. Soon after study there was no difference between memory for the true and the false statements. But after a week the meanings of the true statements were recalled rather better than the meanings of the false statements ($P=0.02$), and the true statements also obtained higher scores on recognition ($P=0.05$). These two results in favour of the true statements were probably due to successful guessing, for identical but more marked effects were shown by the control subjects who had not read the statements ($P=0.002$ or better). In addition, in sixty-four out of the sixty-five instances in which the meaning of the completed statement of a control subject was in some sense the opposite to the meaning of the original statement, the original meaning was false ($P=0.001$). Thus when the control subject guessed, he tended to make up or select a true statement. The experimental subject probably did the same.

Table 2 shows that there were significant differences in memory soon after study between the statement condition and the listening of the previous experiment (Poulton, 1957*a*). Yet in both cases the average rate of presentation was 37 words per minute (one statement every 10 sec.). These differences were associated with a number of major changes in procedure. The data from the listening experiment were only included for the sake of completeness.

We calculated the Tau correlations by subjects between the scores on intelligence test AH4 (Heim, 1955) and the various criteria of memory. Soon after study the correlations tended to be small and positive or negative (in one case significantly negative) after 293 or 146 words had been presented per minute, but about 0.4 after 73 or 37 words had been presented per minute. For delayed memory the correlations tended to be small, and all but one were positive.

IV. DISCUSSION

(i) *Faster reading gave poorer comprehension.* The data in Table 2 (and Fig. 1) for substance memory in the whole condition when 293 and 146 words had to be studied per minute, show the relationship between the approximate speed of reading and comprehension. For (a) substance memory soon after reading is a criterion of comprehension; (b) the subjective reports of re-reading in this condition showed that at these two faster speeds most of the subjects had time to read practically all the statements once and only once (Result (ii)); and (c) the memory scores of the subjects with whom this relationship held least closely, were about the same as the scores of the remaining subjects (Result (ii)). We may therefore conclude from Table 2 that reading at about 293 words per minute gave significantly poorer comprehension than reading at about 146 words per minute.

With longer times for study in the whole condition, Table 3 shows that the subject simply re-read some or all of the statements. The average speed of reading was not reduced below about 130 words per minute in this condition (Result (ii)). The same was probably true in the statement condition, although subjective reports are not available. Thus we cannot use the data from these conditions to determine the effect upon comprehension of reducing the speed of reading below about 146 words per minute. However, the rigid pacing of the segment condition forced the subject to read each statement once only. When 37 words were presented per minute in this condition, the subject had to read at a speed of only about 84 words per minute (see Method (i)). Table 2 shows that recognition soon after study in this subcondition was significantly better than when 146 words were presented per minute, which meant actual reading at about 252 words per minute. Similar trends were shown by the two measures of recall soon after study, but they were not significant statistically.

However, the test of recognition was not as adequate a criterion of comprehension as was recall of the meaning of the statements. As pointed out in the previous paper (see Poulton, 1957*a*, Method (vii)), none of the six forms of the alternative versions of the statements presented the subject with a pure test of recognition of meaning, unconfounded by changes in the words used to carry the meaning. For in order to change the meanings of the alternative versions, some change in wording was necessary. The only way to have got round this difficulty would have been to have changed the wording of both the 'original' and the alternative versions. This would have forced the subject to select the statements with the meanings which were closer to the originals, without being able to recognize specific words from the original statements.

The relationship between speed of reading and comprehension, and the other relationships which we shall soon discuss, were all determined for a particular range of levels of reading skill, which was probably not a very high one. Thus, an extensive course of training before the experiment would presumably have improved the scores of most of the subjects. However, it seems unlikely that training would have altered the *direction* of these relationships. (This could be checked on suitably-trained subjects.)

(ii) *When time was short, careful study was not a disadvantage.* The lower broken curve in Fig. 1 shows that, in the shortened condition when only 3 min. were available for study, the total amount of substance memory soon afterwards was about the same whether all 144 statements had to be studied (293 words to be studied per minute), or whether only thirty-six statements had had to be studied (73 words to be studied per minute). The lower half of Table 4 shows that the same relationship held for recognition. It was only the total amount of memory of the specific words presented which increased consistently as more statements had to be studied in the time.

These results suggest that there is a limit to the amount of material which can be understood in a given time. If we proceed faster than this, we may be able to recall slightly more words; but we shall not be able to recall more of the meaning, nor to recognize more on a test of recognition similar to the one used here. To use the terminology of information theory (Quastler, 1955), the rate of coding the information in reading appears to have an upper limit, which is reached before the upper limit on the rate at which information can be stored in an uncoded form.

The results in the lower half of Table 4 were obtained by simply allocating different blocks of statements to different subjects, and then computing their scores for all the statements, whether they had studied them or not (see Calculations). If certain statements were more important than others, and the subjects knew how to select these statements, careful study of these statements should prove a positive advantage. This seems pretty obvious, and the top half of Table 5 confirms this impression. However, when 293 words had to be studied per minute, it was only recognition soon after study which benefited significantly from selection. Recall soon after study and delayed memory only benefited significantly when fewer words had to be studied per minute. This result is in line with the findings of the previous paper (see Poulton, 1957*a*, Discussion (vi)): recognition tends to become a more sensitive measure as conditions become more difficult.

If this result can be generalized to everyday reading, it suggests that reading selectively at high speed will only be an advantage over reading unselectively, if soon afterwards the reader returns to the information which he has selected (as, for example, in taking notes after reading). In the selection condition when 146 words or less had to be studied per minute, most subjects reported that they did in fact re-read at least once some or all of the selected statements (Table 3). But trainees on courses designed to improve speed and comprehension in reading are not necessarily warned to go over the important points a second time. On these courses a test of recognition normally follows immediately after the reading of a prose passage, and thus takes the place of reference back to the text (Perry & Whitlock, 1948*a, b*). If the present view is correct, the trainees ought to be instructed to substitute in their normal reading some kind of recapitulation for the test of recognition.

(This discussion would not be complete without pointing out the advantage to the reader who has not much time of clear short summaries and meaningful subtitles, for example, Experiment on —, *not* Experiment I. For this reduces the amount of essential reading and selection. A number of general points of this kind for the authors of papers have been made by Howie (1951)).

(iii) *Rigid pacing sometimes helped.* Table 2 shows that when 146 words had to be studied per minute, memory soon after study was significantly better with the rigid pacing of the segment condition, than with the loose pacing of the whole condition. This effect is un-

likely to have been due to a chance difference between the groups of subjects allocated to the two conditions, for two reasons. First, the distributions of scores on intelligence test AH4 (Heim, 1955) were almost identical for the two groups (Result (iv)). Secondly, recall showed a similar trend soon after 73 words had been presented per minute, although this trend was smaller and not significant (Table 2).

Two other explanations of this effect can also be ruled out: (a) that the automatic reading pacer used in the segment condition simply added novelty to the experiment, and thus motivated the subject more highly; and (b) that the whole condition confronted the subject with the additional task of ensuring that he reached the end of the list of statements on time. For first, when 146 words had to be studied per minute, the segment condition proved significantly better than the statement condition. This was the case both for recall of the individual statements soon after study (Table 2), and for the estimation of the proportion of statements 'connected with the navy and life at sea' (Result (vii)). Yet (a) both these two conditions involved using the automatic reading pacer; and (b) neither of them involved the additional task. Secondly, soon after study there were no significant differences which could not be attributed to chance between the whole and statement conditions (Result (iv)). Yet (a) the statement condition employed the reading pacer, while the whole condition did not; and (b) the whole condition involved the additional task, while the statement condition did not.

The difference between the segment and whole conditions when 146 words had to be studied per minute thus suggests that rigid pacing itself helped. In the segment condition at this rate of presentation, the subject had to read at about 252 words per minute for about 1.5 sec., and then had a break of about 1.0 sec. before the next statement was presented (see Method (i)). During this time he could memorize the statement which he had just read. This procedure appears to have been more effective than reading more or less continuously at about 146 words per minute, which was the procedure apparently used by most of the subjects in the whole condition (Result (ii)), and probably also in the statement condition.

Table 2 also shows that when only 37 words had to be studied per minute, the nature of the pacing had no effect upon memory. Yet in the segment condition each statement had to be read once only at an average speed of 84 words per minute (see Method (i)), and the reading was followed by an average interval of 5.6 sec. during which no print could be seen. Whereas in the whole condition each statement was read on average about three and a half times (Table 3), and the same was probably true in the statement condition. Clearly at a rate of presentation as slow as 37 words per minute, memory must have depended principally upon some factor related more closely to the overall time available than to the method of study.

This work was carried out in the Applied Psychology Research Unit under the direction of Dr N. H. Mackworth. I am also indebted to Sir Frederic Bartlett, F.R.S., and to Miss K. D. Napier for their interest. Mr M. Stone gave advice on statistical problems and Dr J. F. Mackworth on presentation. The Royal Navy supplied the subjects, and also the assistance of a sickberth attendant when I had to be in two places at once. Financial support from the Medical Research Council is also gratefully acknowledged.

REFERENCES

- ISHER, R. A. (1948). *Statistical Methods for Research Workers*. Edinburgh: Oliver and Boyd.
- LOCK, M. D. (1949). The effect upon eye-movements and reading rate at the college level of three methods of training. *J. Educ. Psychol.* **40**, 93-106.
- LEIM, A. W. (1955). *Manual for Group Test of General Intelligence AH 4*. London: National Foundation for Educational Research.
- LOWIE, J. W. (1951). On writing to be read. *Lancet*, **261**, 320-1.
- MAUER, A. R. (1936). An experimental study of the improvement in reading by college students. *J. Educ. Psychol.* **27**, 655-62.
- MERRY, W. C., JR. (1955). Personal communication.
- MERRY, W. C., JR. & WHITLOCK, C. P. (1948*a*). *Harvard University Reading Films, Series Two*. Cambridge, Mass.: Harvard University Press.
- MERRY, W. C., JR. & WHITLOCK, C. P. (1948*b*). *Selections for Improving Speed of Comprehension*. Cambridge, Mass.: Harvard University Press.
- POULTON, E. C. (1957*a*). Previous knowledge and memory. *Brit. J. Psychol.* **48**, 259-70.
- POULTON, E. C. (1957*b*). Time for reading and memory. Cambridge: *Appl. Psychol. Unit Rep.* no. 302. (Available from the author.)
- QUASTLER, H. (Ed.) (1955). *Information Theory in Psychology*. Glencoe, Ill.: Free Press.
- SAKODA, J. M., COHEN, B. H. & BEALL, G. (1954). Tests of significance for a series of statistical tests. *Psychol. Bull.* **51**, 172-5.
- TROUD, J. B. & HENDERSON, M. (1943). Rate of reading and learning by reading. *J. Educ. Psychol.* **34**, 193-205.
- WINKER, M. A. (1939). Speed versus comprehension in reading as affected by level of difficulty. *J. Educ. Psychol.* **30**, 81-94.
- WINKER, M. A. (1940). Dr Robinson on speed versus comprehension in reading: a discussion. *J. Educ. Psychol.* **31**, 559-60.
- WOODWORTH, R. S. (1938). *Experimental Psychology*. New York: Holt.

(Manuscript received 14 June 1957)

EFFECTS OF PRACTICE AND REST ON FLUCTUATIONS IN THE MÜLLER-LYER ILLUSION

BY H. J. EYSENCK AND P. SLATER

Institute of Psychiatry, University of London

Fifty subjects are given sixty trials each with an apparatus exhibiting the Müller-Lyer illusion. The first forty trials (divided for analysis into four periods of ten) are uninterrupted; a 30 sec. pause for fixation is followed by a fifth period of ten trials; and a rest pause of 30 min. by a final period. On the group as a whole, practice produces scarcely any effect. The means differ very little from trial to trial, from period to period, and the amount of variation about them tends to remain constant. The effect of the illusion varies greatly on different individuals. The variance between the subjects' means for the experiment as a whole constitutes 63 % of the total. There are also striking differences in the effects of practice on different subjects; these person:period interactions account for another 14 %. The error variance forms almost the whole of the remainder (22 %).

To classify the person:period interactions, a principal component analysis was employed. The first latent root, accounting for 59 % of the variance due to interaction, describes a course of change which retains the same direction for any particular subject (whether upwards or downwards, towards or away from the point of zero illusion) up to the end of the fifth period but is reversed by the rest pause. The authors do not consider the terminology of satiation or improvement with practice suitable for describing the whole range of such phenomena, but suggest a description in terms of habit reinforcement. However, the phenomena exhibited by subsequent components, which cannot be neglected as statistically non-significant, do not seem so explicable. All such performances constitute patterns of search, and it is noteworthy that none of them accord with the oscillatory pattern suggested by cybernetic theory. Scores on scales of neuroticism and extraversion-introversion failed to correlate significantly with individual differences either in the effect of the illusion over the experiment as a whole or in the progressive changes induced by practice.

I. INTRODUCTION

Few experimental variables in the field of learning have been studied more assiduously than the effects of practice and rest upon the development of the particular function under investigation. Experimental work in the field of perception, on the other hand, has not usually been concerned with variables of this type. Possibly due to the phenomenological bias of many workers in this field, massed or spaced repetition of trials has usually only been resorted to in order to increase the reliability of individual observations, and not in order to study the effects of such repetition on the dependent variable.

In previous contributions Eysenck (1955, 1957*a*) has suggested that certain effects in the perceptual field, which Köhler calls *satiation* effects, resemble certain other effects in the field of learning theory, which Hull calls *inhibition* effects. It was further hypothesized that inhibition-satiation effects are more strongly evoked in persons lying on the extraverted end of the extraversion-introversion dimension than those towards the introverted end. This hypothesis has been verified in several studies at an acceptable level of statistical significance (Eysenck, 1957*a*). Satiation phenomena present one of the few instances where the effect of practice upon perception has been studied, and the possibility of finding a link here between learning theory, perception, and personality theory may be worth investigating.

One of the phenomena which may be studied in this connexion is that of the so-called visual illusions. While most interest has, from the beginning, been aroused by the

phenomenon itself and its possible relations to the analytic-synthetic modes of approach of different personality types, several workers have none the less been interested in the course of development taken when the illusion patterns are presented a large number of times. The work of Judd (1902, 1905), Seashore (1908), Lewis (1908) and Brown (1953) on the Müller-Lyer Illusion, that of Cameron & Steele (1905) on the Poggendorf Illusion, and that of Judd & Courten (1905) on the Zoellner Illusion, may be mentioned in this connexion. These writers put forward the general hypothesis that, as Köhler & Fishback (1950*a*, 1950*b*) put it, 'all such illusions represent errors of judgements rather than actual distortion of visual objects. From this point of view, it may seem natural to assume that when subjects deal with the illusion patterns in a great many trials, they gradually learn to avoid these errors, and that the illusions disappear for this reason.' Traditionally, then, although perception is involved in the experiment, the interpretation is in terms of learning, and comparatively little interest would attach to the alleged disappearance of the illusion with practice. But the only type of fluctuation with practice such a simple theory would be adequate to explain, would be a progressive decline in the amount of the illusion from some initially large positive value to a final value approximately zero. This is far from being the only possible origin, direction of change, or termination to be found described in the literature or observed by us.

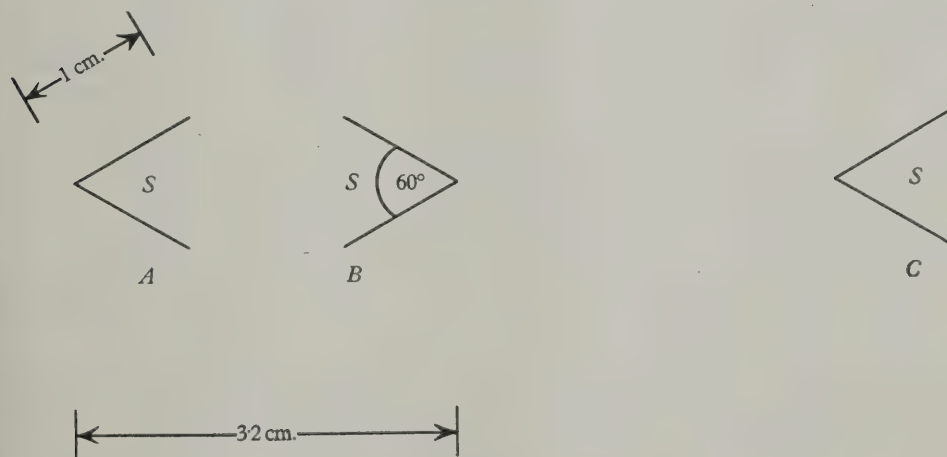


Fig. 1

In a brilliantly argued series of deductions, Köhler & Wallach (1944) and Köhler & Fishback have maintained: (1) that the explanation in terms of learning is inadmissible because subjects do not know where, and to what extent, their judgements are in error, and cannot therefore correct this; and (2) that the phenomenon of the disappearance of the illusion with repeated trials could be accounted for in terms of the general principles of satiation. This explanation, briefly, is that satiation develops more quickly inside the angles formed by the arrowheads, so that, as satiation develops, the apparently shorter distance included within the arrowheads will appear to become extended until it equals, or even exceeds, the distance between the oblique angles. In terms of Fig. 1, satiation develops most strongly at the points marked with an 'S', pushing, in terms of the subject's visual experience, point A to the left, point B to the right and point C to the left.

So long as the only phenomenon to be explained is a progressive diminution in the effect of the illusion with practice, a choice between 'satiation' and 'learning' as descriptive terms cannot be guided by the evidence, and must rest on the relative strength of the logical arguments in support. But Köhler and his colleagues note a wide variety of other phenomena, and while they adduce such evidence as in conflict with the learning theory, they run into difficulties in attempting to extend their own satiation theory to accommodate it. Particularly with massed practice, and at least in some people, it has been reported that the illusion *increases* rather than *decreases* with time. This is acknowledged by Köhler and Fishback when they say: 'The obstacle which develops when many trials are given without rest periods does not merely delay the process by which the illusion is destroyed. Often its disturbing effect becomes so strong that for a while the direction of the development is actually reversed.' They also quote Seashore's report that one of his subjects showed no change in the size of the illusion even after measurements had been made for 24 days. The modified hypothesis they propose to account for such phenomena is by no means clear-cut and obvious. They point out (footnote on p. 274) that at first sight it might appear that, in such subjects, satiation must be exceptionally weak or that it must fade particularly fast, 'but if we are not mistaken one could also make the opposite assumption. There can be little doubt that, as a consequence of everyday vision, the visual cortex as a whole is in a state of quasi-permanent satiation, and that local satiation as established in special fixation periods is merely added to the level of that persistent condition. . . . Now satiation is a self-limiting process. . . . It therefore seems at least possible that, with some subjects, temporary and local satiation effects are found to be weak because the more persistent satiation of the tissue as a whole is unusually strong.'

We find it very difficult to operate with this modified hypothesis. It seems to mediate alternative expectations without specifying the conditions under which one or another is to be preferred, and thus to be of a kind repugnant to science. Moreover, the properties Köhler attributes to satiation are not at all in agreement with common findings which are that satiation effects disappear relatively rapidly. Long-lasting effects are much more typical of learning phenomena.

Köhler & Fishback's major objection, that when subjects are precluded from comparing their results with the objective situation they cannot learn, implies a restriction to the connotation of the term learning which may not be psychologically tenable. Learning in the sense of habit formation may occur without any such facilities, as long as a *reinforcement* is provided. Köhler himself, in a footnote on p. 278 of his paper with Fishback (footnote 13), has given a clue as to a possible source of reinforcement and our own observations are in agreement with his. This is what he writes: 'When subjects have sufficient time to do so, they will usually try to make sure that their first impression is reliable, and will therefore postpone their final decision. Since, during this period, satiation can change the appearance of the pattern, they may find themselves disturbed by contradictory impressions.' We have frequently found in our experiments that a subject may become discontented with the setting he has chosen while the experimenter is writing it down; if so, a reinforcement is provided for a change in that direction on the next occasion, and a habitual trend of change may thus be built up. This may be towards a decrease in the illusion if the initial effect is positive and the direction taken is downwards; but even so it may continue beyond the point of objective equality and terminate in a negative effect. Or the trend may equally well be in the opposite direction. The relation-

ships of such trends to the point of equality will differ according to their starting points, directions and rates of change; but they may all be instances of habit formation.

However, if an ethical implication is considered inseparable from the term learning, in the sense that, at the end of a learning process, performance must in some way be *better* than at the beginning, its application to the patterns of behaviour we are considering must be withdrawn. In that case, it might be better to employ the term exploration to describe them. The adoption of a plan of search is found to be normal in subjects with a mental age of 13 years or over; and it may therefore be normal for an adult subject, dissatisfied with a particular setting during a series of trials with the Müller-Lyer illusion, to try modifying it systematically rather than at random. What we are suggesting then, is that, among such subjects, an orderly trend is observable in exploratory behaviour, not necessarily following the same pattern in all cases; that it may be maintained by some reinforcing stimulus as long as no obstacle or interruption disturbs it; and that there are consequently some similarities between exploratory and learning processes.

Such theories do not enable a particular sequence of performances to be defined as the most probable one for a particular subject on any evidence available prior to the experiment, but lead to some expectations concerning the performances of groups of subjects which can be verified. No general tendency need be expected for the illusion to increase or decrease during a period of massed practice, nor for the variance about the mean effect to increase or diminish; differences in starting points, directions and rates of change, being free to vary for different subjects, will operate as that multitude of small independent causes which tends to stabilize a normal distribution. But the performances of individual subjects may be expected to show systematic changes, demonstrable by dividing a long series of trials into a few periods of several trials each and testing whether the individual's mean performances differ significantly from one period to another. There is no particular reason for expecting that the systematic changes exhibited by different individuals will be related to their scores on measures of extraversion-introversion if the satiation theory is untenable.

On any form of the satiation theory, however much modified, it would seem reasonable to expect some general tendency in a group of subjects towards convergence on the point of objective equality at some time during a long series of trials, more probably nearer the end. If the initial effect of the illusion is large, it should tend to decrease; if small, to remain small or decrease further, possibly to a negative end-point. The exceptional cases should be relatively few.

II. EXPERIMENT

(a) *Subjects.* Fifty male subjects between 18 and 25 constituted the experimental group; nearly all were University students and all were experimentally naive and had no knowledge of the aim of the investigation.

(b) *Apparatus.* An illusion pattern was housed in a wooden box, 45 in. long, 4 in. high and 6 in wide. A special face-mask made of rubber enabled the subject to press his face against the open end of the box to the virtual exclusion of all extraneous light, thus reducing external clues. The inside of the box was painted matt black and the pattern was presented to the subject at the other end. It was illuminated from behind by means of a 25 W. lamp. The pattern was drawn on a double sheet of Perspex and was presented with the subjectively shorter distance on the left; this distance was kept constant. The subjectively longer distance could be varied by pulling out the part of the Perspex on which it was drawn and pushing it back again. The dimensions of the illusion, which was presented simply in terms of the angles and without a central line to connect them, were as shown in Fig. 1.

(c) *Technique of measurement.* The experimenter pulled the adjustable part of the apparatus out to a point where preliminary experiments had shown there would be no chance at all of any subject regarding the distance as equal. He then pushed it very slowly back until the subject said 'halt'. His instructions were to call out when the distance between the points of angles *A* and *B* appeared equal to the distance between the points of angles *B* and *C*. The experimenter recorded the amount of error shown by the setting (difference of constant minus adjustable distance in mm.) and removed the adjustable part again to an extreme position somewhat different from that previously used. The subject kept his eyes open throughout and fixated the angle at *B* in Fig. 1. Then the experimenter repeated the experiment immediately, leaving as little time as possible between successive judgements. Forty judgements were made in a row numbered below 1-40. Then the subject was instructed to fixate the illusion pattern by keeping his eye fixed on the point of the angle marked *B* in Fig. 1; this fixation had to be held for 30 sec., after which another ten readings were taken—numbered 41-50. After the fiftieth reading, a 30 min. rest pause was introduced; then another ten readings were taken, numbered 51-60.

(d) *Instructions.* The subjects were told throughout to fixate the point of the angle marked *B*. They were not told of the nature of the illusion or the fact that an illusion was being presented to them at all. They were simply asked to estimate the distances between three points at the tips of the angles and to say halt whenever they considered the distances to be equal. They were shown the mechanism before starting and none made any errors during the run of the experiments. All subjects filled in an extraversion-introversion and neuroticism questionnaire, specially designed by one of us (H.J.E.) and described briefly in another publication (Eysenck, 1957*b*). It was considered possible that the 'analytic' and 'synthetic' types of subjects mentioned in the literature might be found to differ from each other in one or both of the personality dimensions, but no scale relating directly to this trait was used.

III. DESCRIPTION AND ANALYSIS OF THE RESULTS

The group mean for the entire experiment, i.e. the mean of all 3000 measurements, is 1.551 mm., and the sum of squares of deviations from it is 66,106.09 (cf. Table 2). The average effect of the illusion is thus not great compared with the amount of variation about it. Readings range from +16 to -14 mm. and 39.8% are negative. Their standard

Table 1. *Results illustrating periodic fluctuations in the effects of the illusion*

During period	All subjects		Means of subjects		
	Mean	S.D.	S1	S2	S3
1	1.14	4.88	9.8	2.9	-8.4
2	1.28	4.78	9.2	4.0	-8.6
3	1.75	4.44	4.2	2.7	-6.8
4	2.13	4.61	1.7	4.5	-3.9
5	1.56	4.90	0.2	3.4	-1.4
6	1.43	4.48	2.0	3.1	-5.6
All periods	1.55	4.69	4.52	3.43	-5.78

Table 2. *Analysis of the variance of the measurements*

Source	Sum of squares	Degrees of freedom	Mean square variance
Total variance	66,106.09	2999	850.40
Variance between—Persons	41,669.53	49	
Periods	315.57	5	63.11
Person:period interactions			
First latent root	5620.50	49	114.70
Second latent root	1991.07	49	40.63
Third latent root	960.14	49	19.59
Fourth latent root	616.17	49	12.57
Fifth latent root	302.99	49	6.18
Total	9,488.99	245	38.73
Variance between trials within period	248.80	54	4.61
Person:trial interactions within period	14,383.20	2646	5.44

deviation is 4.692 mm. The form of the distribution approximates to the normal, though it does not conform exactly. The value of β_1 , -0.1933 , indicates slight negative skewing (mean below mode), the maximum frequencies occurring at 2 and 3 mm. The value of β_2 , 2.561 , shows that the form is also slightly platykurtic.

The individual means for the entire experiment, obtained by summing the sixty measurements for each subject, have a very wide range, from $+8.90$ to -5.78 mm.; fourteen are negative. Their sum of squares, $41,669.53$, accounts for 63% of the total variance. Subjects thus differ greatly in their susceptibility to the illusion; and such individual differences are the most important observations for any adequate theory of the Müller-Lyer illusion to explain. Their relationship to our personality measurements is very slight, if any: they correlate 0.1860 with the extraversion scale and -0.1151 with the scale for neuroticism. Neither correlation is significant for fifty cases.

The literature on the relation between susceptibility to the illusion and personality (Schiller, 1942) suggests that it is the synthetic type of person who is susceptible to the illusion as opposed to the analytic type. Personality descriptions given by continental authors suggest that the synthetic type is more extraverted than the analytic type, as well as less neurotic. The direction of the two correlations is in line with this hypothesis, but only the former appears worthy of further investigation, being almost significant on the one-tail test. Accordingly, our subjects were divided into three groups, made up of the ten most extraverted subjects ($E+$), the ten least extraverted subjects ($E-$), and the intermediate thirty subjects ($E0$). The mean errors for these three groups were 3.525 for the $E+$ group, 0.820 for the $E-$ group, and 1.142 for the $E0$ group. This order of the means is in line with the hypothesis. Analysis of variance, however, gives a P value somewhat higher than 0.05 . The data may be suggestive, particularly in view of the fact that the order of the means would agree with prediction by chance only in one case out of six, a fact which is not taken into account by the analysis of variance technique. Further work along these lines might be promising.

To distinguish between the irregularities and the systematic changes in the measurements obtained during the course of the experiment the sixty trials have been divided into six periods of ten. Periods 1 to 4 succeed uninterruptedly; then comes the 30 sec. pause for fixation; then period 5; then the rest pause; then period 6. The variation between periods registers the relatively long-term changes, and the variation between trials within periods the momentary irregularities in individual performances. A comparison of the mean-square variances (M.S.V.) from these two sources excludes some classes of hypothesis altogether, though it leaves the choice between others open.

If the two M.S.V.'s do not differ significantly, no evidence of any systematic component is disclosed. If the M.S.V. between periods is significantly greater than the M.S.V. between trials within period, the evidence is consistent with some progressive change of the kind discussed in the introduction. If the latter exceeds the former, oscillation of some kind is suggested: this might be consistent with an attempt to arrive at the true length by cybernetic methods, overshooting it alternately in opposite directions.

In calculating the total variance (sum of squares) for this comparison, the sum assigned in Table 2 to person:period interactions should be included with the variance between periods; and the person:trial interactions within period are to be included with the variance between trials within period. The two M.S.V.'s are 39.22 with 250 D.F. and 5.42 with 2700 D.F., respectively. As the former is very much the greater, progressive change of some kind is indicated; neither of the alternative kinds of hypothesis is acceptable.

If a single direction or course (not necessarily common to all the subjects or rectilinear over the entire experiment, but sufficiently common and consistent to be statistically

significant) is to be found in the progressive changes in the measurements obtained from different subjects during successive periods, it will be shown in the group means per period. But if the directions taken by different subjects vary independently, they will tend to obliterate one another, and the variation in the group means will tend to be small and erratic. Thus consistent changes will augment the variance between periods and independent changes will augment the person:period interactions.

The observed effects are illustrated in Table 1, where the period means for the first three subjects are tabulated alongside the means for the entire group. S1, as can be seen, is markedly affected by the illusion at the beginning of the experiment; as it proceeds, the illusion appears to wear off and during period 5 its effect is very slight; after the rest pause it appears to have returned, though not in its original strength. A graph of this performance would resemble Köhler's Fig. 3 (1950*a*) very closely. S3's performance is almost the exact reverse: he starts with a substantial underestimate of the distance the illusion supposedly lengthens; his estimates increase as the experiment proceeds, approaching the true value most closely again at period 5; and again, after the rest pause, there is a relapse. But no such course is followed by S2; in his case the illusion appears to be comparatively stable. The group means, which are the net resultant of such divergent courses, also show little fluctuation.

That the courses followed by different subjects vary from one another and do not cumulatively define any single predominant direction of change is shown by the fact that the *M.S.V.* between periods (due to differences in the group means per period) is not significantly greater than the *M.S.V.* of the person:period interactions. This disposes of the simpler forms of the learning and the satiation theories discussed in the introduction, together with any other theories which postulate that a single course of change will be found to predominate during a period of massed practice. Even if it succeeded in accounting exactly for all the variations in mean measurements from trial to trial as well as from period to period, any such theory would leave over 99% of the total variance unaccounted for: the variance between the sixty trials is only 0.85% of the total.

To discover any common characteristics among the courses followed by different subjects it is necessary to analyse the person:period interactions. Next to the differences between individual experimental means, they are the largest source of variance outside the experimental error, comprising 14.4% of the total. Detailed tabulations were made. For example, S1's mean for the entire experiment is +4.52 (cf. Table 1); it exceeds the group mean by 2.97 mm. If his course had conformed with the group's, his means per period would have been 4.11, 4.25, 4.72, 5.10, 4.53 and 4.40. The deviations of his observed means from these expectations, viz: +5.69, +4.95... -2.40, measure his period interactions. Tabulating them for each subject provides a matrix of 6×50 measurements with, however, only 5×49 D.F. between them. Significant interactions were not found to be confined to a few of the subjects. They lie beyond the 0.01 probability limit in thirty-three cases and between the 0.05 and 0.01 limits in three more; it is the absence of any interaction, as in the case of S2, which is unusual.

Their variances and covariances per period are given in Table 3. Looking along the leading diagonal it is noticeable that the largest deviations occur towards the beginning and end of the experiment; the smallest variance is at period 3. Deviations in the first three periods tend to correlate positively with one another and negatively with those in subsequent periods. There is evidently a general tendency for the subjects to give responses

at the start of the experiment which lie at a considerable distance away from what subsequently prove to be their mean points for the experiment as a whole; to converge on these points as the experiment proceeds, coming nearest usually in period 3; and continuing roughly in the same direction to move on past them away again. Subjects 1 and 3 (cf. Table 1) follow courses which conform approximately with this description.

Table 3. *Matrix of the total variances and covariances* of the person:period interactions*

3309.5	762.5	-262.7	-1574.1	-1737.2	-498.4
—	1092.5	109.5	-456.8	-910.6	-597.6
—	—	617.5	291.5	-306.5	-448.8
—	—	—	1359.3	331.6	47.8
—	—	—	—	2118.8	503.9
—	—	—	—	—	993.3

* Sums of squares and products, with 49 D.F.

It might be supposed, if this is the general tendency, that there will be some point during the experiment when the measurements obtained lie closer, if not to the true value, at least to the experimental mean for the group. But this is not the case. As the S.D.'s noted in Table 1 show, the amount of variation about the period mean for this group is approximately the same at all stages during the experiment. This is because the convergence which tends to occur over the first thirty trials is of the individual's responses towards their own experimental means, which vary widely about the mean of the group; nor is period 3 always the point at which convergence is closest; and indeed persistence in the same direction does not remain uninterrupted in every case.

To reduce vagueness in discussing what general tendency or tendencies are to be found among these interactions a technique is needed for abstracting common characteristics and measuring their importance. An adaptation of principal component analysis is submitted by one of us (P.S.) for this purpose. It leads to the latent roots of the components included in Table 2 and the weights proportionate to factor loadings given in Table 4.

Table 4. *Latent vectors of the person:period interactions**

CI	CII	CIII	CIV	CV
0.731	-0.309	0.107	-0.371	-0.231
0.279	0.311	-0.421	0.686	-0.107
-0.007	0.429	-0.102	-0.387	0.700
-0.341	0.469	0.413	-0.150	-0.552
-0.492	-0.463	-0.560	-0.201	-0.151
-0.171	-0.436	0.563	0.424	0.344

* Scaled appropriately for calculating component measures from preceding residuals. To obtain loadings for extracting components from the matrix of total variances and covariances (Table 3) multiply each vector set by the square root of its latent root, given in Table 2.

When multiplied into S1's interaction measurements, the weight for the first component gives his C1 index, thus:

$$5.69 \times 0.73 + \dots + (-2.40) \times (-0.17) = 9.244$$

and the C1 index, multiplied by the weights, gives the modified expectation of his performance during the successive periods illustrated in Fig. 2. The fit is evidently improved substantially in this case.

The C1 index for the group as a whole varies about 0 with a S.D. of 3.387, i.e. (5620.50 $\div$ 490)[†]. Particular values designate individual specimens of the same general type of

performance, analogous to members of a family of curves designated by a particular choice of constants. A positive index describes a course dropping, like S1's, to a trough at period 5, but the point of the trough may be located anywhere above or below the base line of equality (no illusion). Conversely a negative index describes a course rising, like S3's, to a crest at period 5; and this too may be located anywhere above or below the base line. The position of trough or crest relatively to the base line depends, for any given CI index, on the subject's experimental mean.

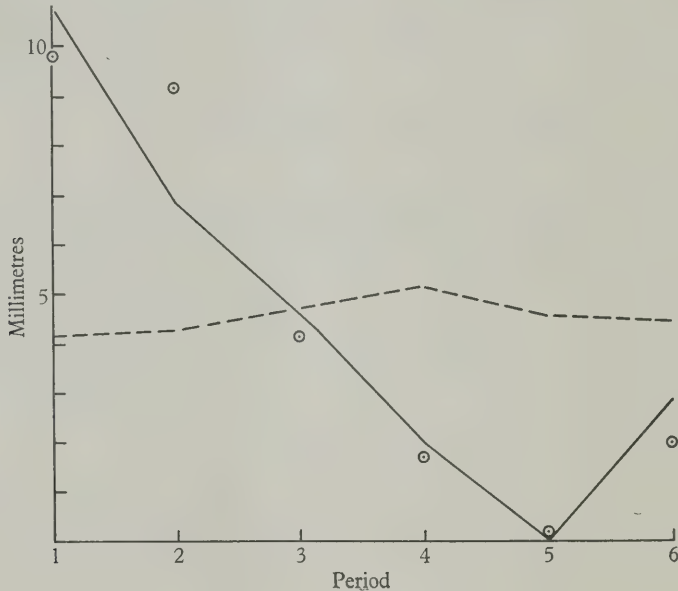


Fig. 2. Period means for S1. $\odot$, Observed; ---, expected from group means; —, expected taking CI into account.

While the terminology of satiation may apply approximately to the course followed by S1, trying to extend it to S3 may be felt to strain it. The terminology of learning, in the sense of improvement with practice, might apply equally well to both subjects' performances; but it too would be strained if extended to cases where a positive experimental mean occurs concomitantly with a negative CI index, or a negative mean with a positive CI index, i.e. where the course towards the crest or trough takes the subject *away* from the base line. Such occurrences could be readily exemplified from our data, and the statistical analysis shows that they can be regularly expected and all referred to a common form. However, in terms of the modified learning theory outlined in the introduction, that is to say as phenomena of simple habit reinforcement, all courses of the CI type are equally explicable, and the reversal of direction which occurs after the 30 min. rest pause is also intelligible. If a psychological description is to be attempted, these terms would seem preferable.

Expectations derived from CI naturally do not coincide with the observed measurements at every point in every case; the deviations from expectation give rise to a matrix of residual variances and covariances from which a second principal component, CII, can be extracted. The process concludes only after the extraction of five components. Just

as the CI index modifies the expectations based only on the experimental and group means, the CII index modifies again the modifications introduced by CI until, finally, with CV the whole variance of the interactions is taken up, and an exact fit is obtained to every subject's observed mean performance per period. The significance of the improvement in fit provided by each modification is tested in the analysis of variance (Table 2). CV is the only component that falls short of significance at the 0.01 probability level.

There thus appear to be types of course which deviate significantly from CI and are not readily ascribable to habit reinforcement. In particular, the 30 sec. fixation period produces startling changes in courses of both the CII and CIII type which are not readily explicable in terms of any theory we have discussed. In accordance with satiation theory the prevalent tendency (downwards towards the base line) should be accelerated, while in terms of simple habit reinforcement no large change is to be expected: but it is evident from CII and CIII that complete reversals are quite common. While all the components could be described as classifications of common plans of search, it is not immediately evident that such a terminology will be helpful for purposes of inference.

One possible hypothesis to account for the reversal in the direction of change during the fixation period might be constructed with reference to reactive inhibition of those cortical elements active in the attention and judgement processes which are supposed to characterize the 'analytic' type of person. Such inhibition might be supposed to accumulate during the forty massed practice trials preceding the 30 sec. inspection period, and, apparently, to dissipate during this period. Experimental evidence is available to show the existence of reactive inhibition in relation to such cortical processes and a direction test of this hypothesis would not seem to be impossible (Eysenck, 1957*a*).

There is no evidence of any significant association between any of the components and scores on the scales for neuroticism or extraversion-introversion. The correlations are

Component	Scale	
	<i>N</i>	<i>E</i>
I	0.0804	0.0376
II	-0.1842	0.1711
III	0.2138	0.0606
IV	0.1905	0.0674
V	0.0871	-0.0864
All components multiple <i>R</i>	0.3605	0.2153

The 0.05 probability limit for a zero correlation is ± 0.2829 in a sample of fifty cases and 0.5255 for a multiple correlation based on five independent variables. There may well be some other personality trait or traits with which these components are associated, e.g. the analytic/synthetic difference. But of this we have no evidence. We cannot at the moment say whether the components described have any significance for personality theory at all.

REFERENCES

- BROWN, K. T. (1953). Methodology for studying figural after-effects and practice effects in the Müller-Lyer illusion. *Amer. J. Psychol.* **66**, 629-34.
 CAMERON, E. H. & STEELE, W. N. (1905). The Poggendorf illusion. *Psychol. Rev. (Mon. Suppl.)*, **7**, 83-111.
 EYSENCK, H. J. (1955). Cortical inhibition, figural after-effect, and the theory of personality. *J. Abn. Soc. Psychol.* **51**, 94-106.

- EYSENCK, H. J. (1957*a*). *The Dynamics of Anxiety and Hysteria*. London: Routledge and Kegan Paul.
- EYSENCK, H. J. (1957*b*). The questionnaire measurement of neuroticism and extraversion. *Riv. Psicol.*, **50**, 113-140.
- JUDD, C. H. (1902). Practice and its effects on the perception of illusion. *Psychol. Rev.* **9**, 27-39.
- JUDD, C. H. (1905). The Müller-Lyer illusion. *Psychol. Rev. (Mon. Suppl.)*, **7**, 55-81.
- JUDD, C. H. & COURTEN, H. C. (1905). The Zöllner illusion. *Psychol. Rev. (Mon. Suppl.)*, **7**, 12-39.
- KÖHLER, W. & FISHBACK, J. (1950*a*). The destruction of the Müller-Lyer illusion in repeated trials. I. An examination of two theories. *J. exp. Psychol.* **40**, 267-81.
- KÖHLER, W. & FISHBACK, J. (1950*b*). The destruction of the Müller-Lyer illusion in repeated trials. II. Satiation patterns and memory traces. *J. Exp. Psychol.* **40**, 398-410.
- KÖHLER, W. & WALLACH, H. (1944). Figural after-effects. *Proc. Amer. Phil. Soc.* **88**, 265-357.
- LEWIS, E. O. (1908). The effect of practice on the perception of the Müller-Lyer illusion. *Brit. J. Psychol.* **2**, 294-306.
- SCHILLER, L. (1942). Ganzheitliche Auffassung und Persönlichkeitstypus. *R. Psychol.* **153**, 43-80.
- SEASHORE, C. E. and others (1908). The effects of practice on normal illusions. *Psychol. Rev. (Mon. Suppl.)*, **9**, 103-48.

(Manuscript received 8 July 1957)

PUBLICATIONS RECENTLY RECEIVED

Principles of Perception. By S. HOWARD BARTLEY. New York: Harper. London: Hamish Hamilton. 1958. Pp. xii + 482. 52s.

It is perhaps unfortunate that Dr Bartley chose, as the title of his book, *Principles of Perception*, since although he does enunciate certain useful principles to be observed in studying and hypothesizing about perception, the greater part of the book consists of an introduction to the subject describing some of the experimental investigations which have been carried out. Inevitably only a selection of these could be covered, since all the sensory modes are included; and there is a bias towards experimental work on the physiological psychology of perception, although little purely physiological theory is discussed. Thus, colour vision and hearing are left somewhat in the air. However, obviously the book would have been excessively long had their theory been included.

A more serious fault is the rather scrappy treatment of certain of the psychological phenomena of perception, which seems to be due in part to the omission of almost any reference to the work of other than American psychologists. The psychology of form perception and the Gestalt studies are hardly mentioned, and the work of Piaget and Michotte is omitted altogether. It is ludicrous to devote a chapter to the 'development of perception' with no reference to Piaget. However, there is an excellent critical discussion of the role of social and motivational factors in perception, and particularly of the number and vagueness of the hypothetical constructs, such as 'subception' and 'perceptual defence', which have been introduced in connexion with these studies.

But this book provides a good introduction to the study of perception, provided that it is not regarded as comprehensive. It will be the more valuable if some of the principles stated by the author are understood and applied in studying and performing experimental work on perception. In the first place, he stresses the distinction which should be made between the qualities of the physical stimulus and the nomenclature applied to them, and the qualities of the perceptual experience with its appropriate terminology. Not only must the student have this distinction clearly in mind; but he must also differentiate between the essential discriminatory characteristics of the immediate perception, and those processes appertaining to the further elaborations of judgement and valuation, which in the past have often been confused with them. The trouble, of course, comes in knowing where to place the processes of inference and identification which generally form an integral part of perception; and the author is not always very clear in his discussion of these. He does, however, make the important point that certain characteristics are merely descriptions of the percepts, and cannot be regarded as causal factors determining them. This raises the issue of the functions of 'cues', for instance, 'cues' to the perception of distance. Unfortunately, this issue is not adequately clarified, and no discussion is included of observations made on the effects on spatial perception of prolonged inversion or reversal of the visual field. Instead, the author wanders off into a lengthy discussion of physiological optics which seems to be of little interest to the student of psychology.

Some of the main theories of perception discussed by F. Allport are briefly stated at the beginning of the book and recapitulated in the final chapter. The author points out that though several of these can be applied to certain particular perceptual phenomena, none accounts satisfactorily for all such phenomena. But this seems inevitable and even desirable in the present state of our knowledge. It is far better for the beginning student to be confronted with the gaps and inconsistencies between rival theories than to suppose that there can as yet be any definitive and all-inclusive theory to which all the mass of incoming experimental data must conform.

M. D. VERNON

Contemporary Approaches to Cognition. By J. S. BRUNER *et al.* Harvard University Press. 1957. London: Oxford University Press. 1958. Pp. vi + 210. 32s.

This record of Colorado University's 1955 symposium on cognition contains a short introduction by Muenzinger and six chapters, each by a different author and all but the last followed by a critique from one of the other participants. The first chapter, by the late Egon Brunswick, maps out the place of cognition in that polysyllabic conceptualization of organism-environment interaction made familiar by Brunswick's previous writings. Then follows what is, for the reviewer, perhaps the most stimulating

contribution; Bruner's 'Going beyond the information given'. Taking his cue from Bartlett's schemata, and extending his own work on categorizing, Bruner outlines a scheme which, whatever its limitations, lends unity to a wide array of phenomena. His integrating concept is 'the coding system', i.e. that way in which people assimilate information to organized bodies of past experience and, in terms of this assimilation, process and go beyond what is given. If a person is given the numbers 8 and 4 and asked 'What is the answer?', this answer will depend on whether the thinker superimposes on the data the templet of multiplication, division, addition, subtraction, or continuation of a series: he codes the data so as to extrapolate from them in one way rather than another. This example is trivial and also caricatures the extent to which most problem situations are ambiguous but, in the hands of Bruner, the notion which it illustrates is elegantly applied to much of perceiving, learning and thinking: and in the later clinically oriented chapter by Rapaport on 'Cognitive structures', this same notion may be seen to extend to those quasi-permanent cognitive 'styles' and value-systems of adult personality. The longest chapter is Osgood's bold and sparkling 'Behavioristic analysis of perception and language' which ranges all the way from sensory mechanisms to meaning by a breath-taking *tour de force*. Festinger contributes a cogent discussion of what he calls 'cognitive dissonance' (his book on this topic has been published in the interim), and Fritz Heider rounds off with a chapter on 'Trends in cognitive theory'.

This book is unlikely to be read continuously for many years to come. Nor does it set forth all contemporary approaches to cognition: two notable omissions are Piaget's developmental work and the case-history study of the circumstances leading up to scientific invention and discovery. The book's importance lies in its timeliness. It brings together investigators of varying backgrounds for free and speculative discussion on cognitive processes at a time when psychologists are evincing a growing interest in these processes without, as yet, having split into opposed theoretical camps; and its proceedings may awake in many readers some appreciation of cognitive problems and perhaps influence some psychologists to tackle these problems in a many-sided and non-partisan way. If any facet of human activity is, more than any other, the proper subject matter of psychology, then cognitive activity is surely it. Yet following their early concern with 'higher mental processes', psychologists have, with a few noteworthy exceptions, shown little interest in this facet: the study of reasoning, thinking and imagining has gone through a 'latent period' during most of the first half of the present century. It might, with some justification, be claimed that this latency reflects lack of adequate methodological and conceptual tools and that these tools have to be furnished, somewhat indirectly, from explorations of: traditional sensory and learning laboratory experiments with animals and humans; statistical control of and inference about mental test performances; the properties of mechanical and electronic systems; the often bizarre interpersonal to and fro of psychotherapy; and accumulated naturalistic observation. It might further be claimed that, armed with these tools, psychologists will return to 'higher mental processes' with an attack which is more varied, subtle and fruitful than before. The most forceful overall impression made by this book is that this renewed and more effective attack has begun in earnest.

I. M. L. HUNTER

A Theory of Cognitive Dissonance. By LEON FESTINGER. Illinois: Row, Peterson and Co. 1957. Pp. ix + 291. \$ 5.25.

Where there is inconsistency between the various attitudes and beliefs of an individual there will also be what Festinger calls 'psychological discomfort'. It is this inconsistency resulting in psychological discomfort which is discussed in the book under the label of 'dissonance'. Likewise, the word 'consistency' is replaced by the word 'consonance'. From two basic hypotheses—that where there is dissonance a person will be motivated to try to reduce it, and that when such dissonance is present a person will actively avoid situations and information which would increase the dissonance—Festinger proceeds to develop his theory of cognitive dissonance. Thus the basic data of this theory is common property to all men and some might say furthermore that common sense will take us far in understanding the way in which such dissonance arises and is changed. It is Festinger's conviction that it is possible to make this notion of cognitive dissonance scientifically respectable and moreover that, by avoiding loose usage of the concept of dissonance, it is possible to construct a theory which can make specific predictions which can then be experimentally verified or otherwise. The book is equally divided between theory and data in support of the theory. The data examined in the light of the theory are of two classes: on the one hand, social survey data and, on the other hand, data derived from controlled laboratory experiments with small groups. This reviewer finds the evidence from the laboratory experiments much more unequivocal in support of the theory than the social survey data. Moreover, one is impressed by the ingenuity shown

by Festinger and his collaborators in their attempts at the precise measurement of somewhat nebulous entities. It is refreshing to find an author so ready to claim no more than Festinger does when, in his closing sentence, he writes 'If treated as precisely and specifically as possible, I believe the theory of cognitive dissonance will prove a useful explanatory and predictive device'. This book is to be recommended primarily to those interested in social psychology and sociology rather than in the field of cognitive psychology.

M. A. JEEVES

Introduction to Method in Psychology. By W. M. O'NEIL. Melbourne University Press. London: Cambridge University Press. 1957. Pp. ix + 155. 25s.

This is at once a good and exasperating piece of work. It is good because, in a subject plagued by prattling textbooks, it treats the student as a grown-up, and demands of him mental efforts which are justified by the difficulty of the subject matter and necessary to real understanding. It is exasperating because, though it discusses method and system, it seems itself to have been put together in a rather unsystematic way. A concrete example, the 'moon illusion', is described at the beginning as though it were basic for the rest of the book. It is analysed at length in the sections which follow immediately, and returned to here and there during the later stages. But in the meantime other examples have been chosen, other kinds of problem discussed, and the impression left on the reader at the end is not so much of a single text as of a series of penetrating papers dealing with various aspects of scientific method in general. The range is very wide. Particular techniques are dealt with as well as the logic of experiment, and the syllogism finds a place along with analysis of variance and problems of sampling. The level of difficulty throughout the book is uneven. Sometimes the student has to work really hard, later he may have to deal with quite elementary concepts, and then suddenly he is stretched once more. To get most out of it, and there is a lot to get, he should treat the text as something to be referred to in connexion with particular kinds of problem rather than as a connected treatise to be studied on its own. In this way Prof. O'Neil's thinking can be brought to bear on topics as they arise. There is certainly no alternative study of scientific method in the psychological field which has packed so much into such modest space.

JAMES DREVER

Experimental Design in Psychology and the Medical Sciences. By A. E. MAXWELL. London: Methuen. 1958. Pp. 147. 17s. 6d.

It is seldom that one encounters a book that so clearly calls for no review other than an indication of contents and a strong recommendation. Dr Maxwell's concern is with statistical designs, more particularly those likely to be of use to the clinical psychologist in the testing of hypotheses. All are based ultimately on analysis of variance: correlational techniques and the concept of regression are introduced relatively late in the book, at the point at which they become relevant in relation to analysis of covariance. Two designs are included which, as the author points out, have not hitherto been treated in any textbook; these are designs described originally by D. R. Cox in *Psychometrika* (1951, 1955), the more obviously important of which provides a means of handling differences due to irreversible conditions, e.g. effects of a surgical operation.

To claim that the book reads like a novel is perhaps to exaggerate, but the reviewer was unable to put it down while reading at least Chapter I, with its entertaining 'Anecdote concerning Clocks'; the progressive revelation, too, of how the efficiency of a design can be increased with decrease in error variance is not without its element of suspense. And certainly the simplicity and unity of the principles on which the system is based measure up to the criterion of mathematical elegance.

In short, this is both an important and a pleasing book.

B. SEMEONOFF

Elements of Psychology. By DAVID KRECH and RICHARD S. CRUTCHFIELD. New York: Knopf. 1958. Pp. xxi + 700 + xxxvi. \$ 8.75.

This is a large new introductory text written mainly from a phenomenological point of view but covering the whole of psychology. The book is divided into four parts headed *Perception* (184 pp.), *Motivation and Emotion* (158 pp.), *Adaptive Behaviour* (148 pp.) and *The Individual* (188 pp.). The parts comprise twenty-five chapters, three of them dealing with physiology and one with psychological measurement.

The contents of the book should please those who are tired of neobehaviourism, learning theory, operationism, and the like. Wherever possible the primary treatment of each topic is phenomenological and, by preference, the authors relate their terms directly to conscious experience rather than to behaviour. As expected, this approach is most successful in dealing with perception and problem solving. The chapter on emotions consists mostly of a classification and the description of conscious states of grief, shame, delight, loneliness, etc. Motivation is dealt with mainly in terms of perceived needs and desires although it is acknowledged that there may be discrepancies between the motive a subject reports and what an external observer would judge his motive to be. In discussing learning, the conditioned response is treated as anticipatory, adaptive, and characterized by 'relative lack of awareness and control'; rote learning is given a rather distinctive perceptually oriented treatment; trial and error learning is taken to be a process of cognitive discovery; human skills are regarded as improving with practice due to the acquisition of 'kinesthetic knowledge'. A distinction is drawn between 'Habitual Behavior', consisting of well-practised sequences of responses going off with a minimum of awareness and 'Coping Behavior', concerned with discovering and attaining goals, and involving attention. Possibly, as the result of concentrating on mental events, the chapters on physiology do not form an integral part of the text. Significantly, these chapters occur at the end of each section. The forty pages on the physiological basis of perception do not seem to be needed at all for the discussion of perception that precedes them. A chapter on measurement effectively examines some problems of scaling and sampling and presents elementary statistics, although it is a little unbusiness-like in that it gives a full discussion of the standard error at the end of twenty-eight pages, but does not even mention significance levels or critical ratios. The remaining chapters, concerned with topics such as growth, intelligence, and personality are all interesting and competent.

One of the best features of the presentation is the use of 'boxes'—material set in smaller type and segregated from the rest of the text. There are 169 such 'boxes' mostly containing reports of experiments and aiming to provide enough detail for the reader to be able to evaluate some of the generalizations that appear in the main body of the book. Line illustrations of a serious and helpful type have been used freely. Informal summaries are frequent and each chapter ends with a full glossary that might be valuable for self-testing. The style of writing is more modest than that of the earlier book by the same authors although words are still used lavishly.

As an introductory text, the book is attractive for a number of reasons. It acquaints the reader with a wide variety of current and 'classical' research; its presentation is intrinsically interesting and serious in tone; its approach is probably more meaningful and acceptable to the beginner than neo-behaviourism; it contains a good deal of sound commonsense. However, as basic introductory reading it is expensive, and even those who favour phenomenology may find that it is in some ways too definitely the product of its authors to be fully satisfactory. Some common terms are given unusual definitions, for example, needs are defined as *experiences* of deficiency; and a number of generalizations, for example, on the nature of discrimination learning, are stated without even a warning that they are controversial.

On the whole, it is probably safe to suppose that those who liked the earlier text on social psychology will like this new book also.

R. L. REID

The Unconscious: a Conceptual Analysis. By A. C. MACINTYRE. London: Routledge and Kegan Paul. New York: Humanities Press. 1958. Pp. ix + 100. 11s. 6d.

Oppressed as we are by long and unreadable books it seems a little churlish to criticize this short and stimulating essay for its sins of omission. But useful as it is, it is not quite useful enough. In a prolegomena and four brief chapters, Mr MacIntyre underlines the extent to which Freud was apt to treat 'the unconscious' as an entity. He then argues that, stripped of substantial peculiarities and re-expressed in adverbial or adjectival form, concepts such as 'repression' and 'unconscious functioning' are so inter-related that Freud's contribution to theoretical psychology should be seen as brilliant and insightful redescription rather than explanation. In his last chapter, Mr MacIntyre seeks to reinforce this view by maintaining that Freudian theory is, in certain respects, incapable of falsification and therefore cannot be granted the status of an explanatory hypothesis.

While all this is stimulating, is it in fact true and if true does it amount to very much? Psychologists have known for long that Freud inclined too much to think in spatial metaphor. This Mr MacIntyre relates neatly to Freud's early neurology and to traces of Cartesian and Lockian views of the 'mind' and 'ideas'. But the nineteenth-century background on which Freud avowedly drew was richer and more

interesting than might be supposed from Mr MacIntyre's pages. Having gone so far he ought possibly to have described the background more fully. The same type of comment applies to the fairly obvious point that the determinism Freud derived from nineteenth-century physics simply doesn't fit with the enlightened self-determination he sought as a result of therapy. But there is more than a simple logical fallacy involved in Freud's position.

More fundamental is the status of Freudian theory as an explanatory hypothesis. Mr MacIntyre may be right in thinking that what we have is brilliant description entangled with views about the interrelations of childhood and adult experience that can be established only by still more empirical inquiry. But, to this reviewer at any rate, the nature of Mr MacIntyre's distinction between describing and explaining is not clear. Hence, it is not clear that to say that Freud 'described' is quite so fundamental a comment as might be supposed. Neither is it clear why, in his last chapter, Mr MacIntyre is so easily frightened by the familiar bogeys of suggestion, imaginary recollection, and ambivalent love and hate, into thinking that a Freudian interpretation could never be falsified with reasonable probability in a given case. One suspects that 'reasonable probability' as the upshot of inquiry is something which Mr MacIntyre would find unsatisfying.

But to make these comments is perhaps to ask Mr MacIntyre to have written a different book, instead of being grateful, as one must be, to a philosopher for stating some of his perplexities so well and so shortly that they must present a challenge to needlessly muddled thought.

JOAN WYNN REEVES

Personality and Temperament. By SOLOMON DIAMOND. New York: Harper Bros. London: Hamish Hamilton. 1958. Pp. vii + 463. 48s.

In the earlier part of this book, the author is concerned to show that what he calls 'fearfulness', 'aggressivity', 'affiliativeness' and 'inhibitory control' are fundamental dimensions of temperament in both animals and humans. Several chapters are devoted to animals, which is probably no bad thing. There appears to be some confusion over motivation, when it is said that 'drive' depends upon physical tension arising from a 'basic life process', while 'motive' is the result of learning—thus the "drive" is primary and the "motive" is secondary, while "motivation" embraces both'. A good deal of attention is given to Sheldon's typology, which the author thinks has been unduly neglected. Relatively little attention is given to evaluating techniques for assessing personality, and this being so, perhaps the MMPI comes in for a disproportionate share. Nor is much attention given to the moral aspects of personality, except in a rather superficial way. Of the later chapters, that dealing with the formation of the self-concept curiously lacks any mention of George Mead and any reference to role-playing. Freud, Dollard and Miller, the neo-Freudians, Lewin, and Jung are discussed, but not in a very well-integrated way. In the final chapter on anxiety and integration, several theories are mentioned, but there is no real attempt to assess or integrate these, though the author seems to have a preference for Freud. This relative lack of critical assessment, indeed, is to be found in other chapters as well, as for example in the discussion of the effects of 'emotional deprivation' in infancy.

There is a marked paucity of references to work other than American, and some expressions, such as a reference to *The Bridge of San Luis Rey* as 'Wilder's beautiful novelette' will irritate many British (and no doubt other) readers. On the whole the book, especially the later part, seems to suffer from lack of unity and continuity, and almost creates the impression of consisting of a series of more or less independent contributions, of somewhat uneven merit.

D. GRAHAM

Homosexuality: its Nature, Causation and Treatment. By CLIFFORD ALLEN. London: Staples. 1958. Pp. 143. 15s.

Homosexuality. Ed. by CHARLES BERG and A. M. KRICH. London: Allen and Unwin. 1958. Pp. 415. 30s.

In the last few years a spate of books on sexual perversion and in particular on homosexuality has risen on both sides of the Atlantic. In Britain this has served to supply the demand created by the publication of the Report of the Departmental Committee on Homosexuality and Prostitution, a body which owed its existence to the public and Parliamentary concern aroused by some sensational criminal prosecutions and convictions for homosexual offences. In this sense, Dr Allen's book can be regarded as, to some extent, a tract for the psychiatric times designed for the lay reader. Having briefly surveyed genetic and endo-

crinological theories of causation, the author expresses his view that homosexuality is essentially a psychological disorder, capable if taken in time of frequent amelioration or cure by various forms of psychotherapy with or without social treatment. He illustrates his contention by a number of abbreviated case histories describing apparent cures by psychiatric or social methods; the book ends with a summary and critique of the Wolfenden Report.

Written by a psychiatrist with lengthy experience in the diagnosis and treatment of homosexuality, Dr Allen's book will serve to offset somewhat the therapeutic nihilism regarding this condition which is all too common in psychiatric circles. It may also encourage those conflicted homosexuals who happen to read it to seek professional help or guidance—no mean service.

The volume edited by Dr Berg in Britain and in the U.S.A. by Mr Krich is of a different order. It is, in fact, an anthology of contributions to the subject by standard authorities, prefaced by a considered survey from the pen of Dr Berg and a section in which excerpts from the autobiographies, diaries and correspondence of homosexuals present some of the subjective aspects of the problem. This preliminary section incidentally, although appealing in content, can scarcely be regarded as representative of the reactions of that large group of 'non-conflicted' homosexuals who apparently live on terms with their perversion without any major disturbance of their mental function, or, at any rate, no more disturbance than could be found in matched heterosexual groups.

Owing largely to the discriminating selection of papers, the clinical section is unusually rewarding. Most omnibus books in which the articles are commissioned by the editor are extremely patchy in presentation and often end by confusing the reader. Dr Berg's selection, although bringing out differences in the theories and practices of the selected authors, provides a more homogeneous survey. Here are articles by, amongst others, Freud, Jung, Stekel, Ferenczi, Hirschfeld, Henry, Schilder and Bergler covering both the psychobiological and the psychoanalytical approach to the subject. This is an unusually successful venture which deserves a place on the bookshelves not only of the psychiatrist, but of all those who are interested in the individual and social aspects of an extremely complicated problem, one indeed that is largely unsolved and may indeed be insoluble save in its criminological aspects. For it is clear that the forces of social intolerance backed by outmoded legal codes contribute not a little to its refractoriness.

EDWARD GLOVER

Chronic Schizophrenia. By T. FREEMAN, J. L. CAMERON and A. MCGHIE. London: Tavistock. 1958. Pp. x+158. 21s.

This monograph has three main components. The first is a clear, succinct summary, in 10,000 words or so, of the central concepts in the psychopathology of schizophrenia. Although reference is also made to Penfield, Tinbergen, and Lorenz, it draws heavily from Freud and later psychoanalysts, notably Hartmann and Katan, and is more narrowly psychoanalytic in view-point than are the second and third components, to which it serves as an introduction. The second is a description of the chief manifestations of schizophrenia, especially the reduced ability to distinguish self from not-self, the confusion of personal identity and the disorders in perception, thinking and memory. The third is a discussion of methods of treating chronic hospitalized schizophrenic patients, which are based upon an understanding of their difficulties and designed to restore ego-functions.

Following Federn, the authors argue that the patient's reduced capacity to experience himself as a separate and distinct entity, the loss of ego feeling, is the basic disturbance, and that the failure to distinguish internal and external sensations and the disorders in perception, thinking and memory are necessary consequences. The essential functions of the ego, with the associated 'secondary process', break down, they assert, and the 'primary process' is then released from the inhibitory influences of the ego. The emergence of the primary process accounts for many of the symptoms of schizophrenia, amongst other things, for the disturbance in associations and the 'splitting' in the thought processes, which Bleuler regarded as the basic symptoms. From this viewpoint, the authors describe observations which they have made in the course of a prolonged and intensive study of a sample of chronic patients. They do not here report their observations systematically as evidence which can be taken to prove their thesis. In the main, they report striking incidents anecdotally, which they then interpret in order to illustrate the thesis. So doing they make sense of much of the strange behaviour of patients which would otherwise appear unintelligible and go some way towards justifying their claim that, with all its limitations, the psychological view is in a position to furnish an integrated and consistent theory of chronic schizophrenic manifestations, and a theory which offers the possibilities of a rational, non-empirical therapy. All this they do very well. Yet they deliberately refrain from discussing aetiology and do no more than

in at their views on the causes of the breakdown of ego functions. To regard this breakdown as basic, and other symptoms as consequences, allows a clear description of the disease, but whether this ordering of the symptoms is sound and useful can ultimately only be decided when the aetiology is specified.

In their discussion of treatment methods, the authors have kept closer to the psychopathology of the disease than have many of those who have recently written on the therapeutic community, although they cover several of the same points, such as greater freedom, the unlocking of doors, home visits and more meaningful occupation. Those readers who are unfamiliar with the conditions prevailing in the back wards of our mental hospitals may be astonished, indeed shocked, by some of the things which the authors need to say, but conditions to start with being no less primitive in their hospital than elsewhere, they had perhaps better opportunities to discover what are the essentials, and what are the frills. In their view the essentials lie in the patient's relationship with the nurse, and, in the early stages, in the fostering of primary identifications particularly. They attach great importance to non-verbal methods. Much needs to be done, therefore, to revise the nurse's traditional role and to train and sustain her in a new role which requires from her a much greater understanding of her patients' needs.

This short monograph raises many important issues, which deserve further consideration along the lines suggested by its authors and perhaps also along more experimental lines. It is to be hoped that they will find opportunities to elaborate their ideas and then to report them more fully. In the meantime this monograph will serve well as a useful and refreshing introduction.

D. RUSSELL DAVIS

The Patient and the Mental Hospital. Ed. by M. GREENBLATT, D. J. LEVINSON and R. H. WILLIAMS. Glencoe, Illinois: The Free Press. 1957. Pp. xviii + 658. \$ 6.00.

This large book, edited by a psychiatrist, a social psychologist and a sociologist, consists of a collection of papers by many different authors arranged under the headings: 'Mental Hospital Organisation', 'Therapeutic Personnel', 'The Ward' and 'The Patient and the Extra-Hospital World'. It continues, in somewhat the same way, the sort of approach of the now famous book *The Mental Hospital*, by Stanton and Schwarz; and, as might be expected, these two authors are among the contributors. Some of the chapters are bound to be a little embarrassing to those who work in mental hospitals. For example, Prof. Loeb's chapter on 'Role Definition in the Social World of a Psychiatric Hospital' endeavours to place psychiatrists, psychologists, social workers, nurses, occupational therapists, and so on, in some sort of hierarchial prestige order, and having done so finds that 'the peer of the chief nurse is someone low in the medical totem pole—low in the professional categories and departments above nurses'. It would be interesting to hear Prof. Loeb develop this theme to a group of British hospital matrons.

The organization of the larger State mental hospitals in America is so strikingly different from the way that the majority of British mental hospitals are run, that many of the comments made in this book have little relevance this side of the Atlantic. Nevertheless, much of interest is discussed, such as the rival claims of the hospital's maintenance and administration against its therapeutic activities; problems of status and authority, individuality and occupational label, communication and so on. Another interesting topic, on which some experimental work is quoted, is that of 'Custodialism' versus 'Humanism' in hospitals. A scale assessing these two attitudes correlates highly with other authoritarian ideological scales when applied to mental hospital staff.

Various experiments are reported where a team of sociologists have tried to change the staff attitude in some chronic wards, and to introduce new methods and a new atmosphere. As might be expected, striking changes in backward patients are not difficult to produce if the right steps are taken. The problem to demonstrate that the new way is easier or more economic. Shortage of staff is as often the bar to such improvements as is the custodial attitude in the administration of the hospital. This is still almost a virgin field of study for social psychologists.

This book is by no means confined to self-conscious studies of interpersonal relations within the mental hospital community, and is as much concerned with the relationship of the patient to the outside world. It raises problems which we should all be thinking about, and discusses them in a manner refreshingly free from dogma.

RALPH HETHERINGTON

OTHER PUBLICATIONS RECEIVED

- The Sociology of Knowledge: an Essay in Aid of a Deeper Understanding of the History of Ideas.* By WERNER STARK. London: Routledge and Kegan Paul. 1958. Pp. 356. 36s.
- The Door of Serenity: a Study in the Therapeutic Use of Symbol Painting.* By AINSLIE MEARES. London: Faber and Faber. 1958. Pp. 119. 21s.
- Educational and Other Aspects of the 1947 Scottish Mental Survey.* Scottish Council for Research in Education. University of London Press. 1958. Pp. 148. 15s.
- The Student-Physician: Introductory Studies in the Sociology of Medical Education.* Ed. by R. K. MERTON, G. READER and P. L. KENDALL. Harvard University Press. London: Oxford University Press. 1958. Pp. 360. 40s.
- Man and Time: Papers from the Eranos Yearbooks.* Vol. 3. Ed. by J. CAMPBELL. London: Routledge and Kegan Paul. 1958. Pp. 414. 35s.
- Freudianism and the Literary Mind* (2nd ed.). By F. J. HOFFMAN. Louisiana State University Press. London: Oxford University Press. 1958. Pp. 350. 40s.
- The Patient Speaks: Mother Story Verbatim in Psychoanalysis of Allergic Illness.* By H. A. ABRAMSON. New York: Vantage Press. 1956. Pp. 239. \$3.50.
- The Human Hand.* By GÉZA RÉVÉSZ. Tr. by JOHN COHEN. London: Routledge and Kegan Paul. 1958. Pp. 138. 21s.
- Psychology and Religion: West and East.* Volume eleven of the collected works of C. G. JUNG. London: Routledge and Kegan Paul. 1958. Pp. 699. 42s.
- Widows and their Families.* By PETER MARRIS. London: Routledge and Kegan Paul. 1958. Pp. 172. 18s.
- New Ways in Management Training.* By C. SOFER and G. HUTTON. London: Tavistock. 1958. Pp. 127. 15s.
- The Undiscovered Self.* By C. G. JUNG. Tr. by R. F. C. HULL. London: Routledge and Kegan Paul. 1958. Pp. 113. 10s. 6d.
- The Circle of Guilt.* By FREDRIC WERTHAM. London: Dennis Dobson. 1958. Pp. 211. 18s.
- Your Speech Reveals Your Personality.* By D. A. BARBARA. Illinois: Thomas. Oxford: Blackwell. 1958. Pp. 174. 42s.
- Collected Papers: Through Pediatrics to Psychoanalysis.* By D. W. WINNICOTT. London: Tavistock. 1958. Pp. 350. 35s.
- The Matrix of Medicine: Some Social Aspects of Medical Practice.* Ed. by N. MALLESON. London: Pitman Medical Publishing Co. 1958. Pp. 234. 45s.
- Analyzing Psychotherapy.* By S. KATZENELBOGEN. New York: Philosophical Library. 1958. Pp. 126. \$3.00.
- Home Conditions: a Sociomedical Study of 1066 Hospitalized Patients with Skin and Venereal Diseases.* By E. LOMHOLT. Copenhagen: Rosenkilde and Bagger. 1958. Pp. 100. Dan.kr. 14.00.
- Copywriting and its Presentation: a Guide to Advertisement and Sales Letter Writing.* By F. W. JEFKINS. London: Crosby Lockwood. 1958. Pp. 238. 21s.